SCIENCE.

FRIDAY, AUGUST 31, 1883.

SONNET.

The years through which aught that hath life, O Sun!
Hath watched or felt thy rising, what are they
To those vast aeons, when, from night to day,
From dawn to dark, thy circuit thou didst run,
With none to greet thee or regret thee; none
To bless thy glowing harbinger of cloud,
Rose-tinted; none to sigh, when, like a shroud,
The banner of Night proclaimed her victory won?

Yet through that reign of seeming death, so long
To our imperfect ken, the marvellous force
Which means to ends adjusts in Nature's plan
Was bringing to the birth that eye of man
Which now, O Sun, surveys thy farthest course, —
A speck amid the countless starry throng.

John Reade.

NOTES ON THE GEOLOGY OF THE TROAD.

A brief summary of the results derived from the observations made in connection with the Assos expedition.

The terranes of the Troadic peninsula comprise a variety of stratified and massive or eruptive rocks. The former, excepting the most recent deposits, which are not considered in this connection, may be divided into three groups, according to their mineralogical conditions and geological age.

The most ancient group is highly crystallized, and, in all probability, belongs to the mica-schist zone of the 'grundgebirge' or archean formation.

The youngest group, embracing the miocene and pliocene tertiary deposits, is, in part at least. well characterized by its fossils. The middle group is not defined, excepting by the widely separated limits of the other two groups. It embraces rocks which may be paleozoic or pre-paleozoic, as well as others which are probably of cretaceous and eocene age.

The crystalline schists have their greatest development in Mount Ida, of which they form almost the entire mass. They are of many varieties, all conformably interstratified, as if all belonged to the same great terrane.

True gneisses are not abundant, and occur chiefly upon the north side of Mount Ida, under such conditions that they appear to overlie the schistose rocks. In Hagi ouldòuren-dagh the mica is in large part replaced by hornblende, so that the gneiss has a somewhat dioritic aspect.

In the schistose rocks, chiefly amphibolites, hornblende is one of the most widely distributed and abundant minerals. It generally appears as actinolite, and not infrequently constitutes almost the whole of the rock in which it occurs. With amphibole, at times, are associated, besides plagioclase, more or less quartz, epidote, magnetite, titanite, and rutile. True mica-schists are of less common occurrence interstratified with the amphibolites.

Near the centre of Mount Ida, the oldest rocks crop out; and among them are talc-schists, which, by the gradual addition of olivine, pass into small lens-shaped masses composed almost exclusively of the latter mineral. According to the nomenclature of Brogger, this rock should be called olivine-schist. By alteration it gives rise to serpentine with the characteristic reticulated structure which ever marks the serpentine derived from olivine. Occasionally the fibrous serpentine forms veins of considerable size in the adjacent rocks.

The olivine-schist, where purest, has no schistose structure. The passage from talc-schist, in which no olivine occurs, to that composed almost completely of olivine, takes place sometimes within a short distance. The chief mass of the rock, however, is a middle stage between the two extremes, having a distinct schistose structure, and composed for the most part of olivine and talc, besides considerable quantities of pyroxene, as well as other minerals not yet determined. At various intervals throughout the zone of schistose rocks, occur rather coarsely crystalline white limestones.

The structure of Mount Ida is a comparatively simple anticlinal, with so short an axis extending east and west that the upper portion of the mountain is approximately a dome.

The highly crystalline stratified rocks are perhaps the chief topographical determinants of that region. Their position and distribution indicate, that, in the early stages of its development, the peninsula of the Troad was represented by several islands, which furnished much of the detritus for subsequent formations.

The rocks of the middle zone are for the most part semi-crystalline limestones, a very ferruginous quartzite, together with greenish,

somewhat schistose rocks, and others which are macroscopically like argillites, but contain too large a proportion of quartz. The limestone is generally compact, gray or reddish colored, very like the cretaceous (according to Professor Neumayr) in the acropolis at Athens, and has often large quantities of silica so irregularly accumulated as to produce a very rough weathered surface like the cretaceous limestone west of Smyrna. This limestone is found chiefly about the base of Mount Ida, at Edremit, Qojikia-dagh, and Chalỹ-dagh, as well as between Qayalar and Ahmadja, and several kilometres south-west of Ilisfagy. At Qojikia-dagh it is peculiar in containing many small needle-shaped quartz crystals. The ferruginous quartzite was observed only upon the acute summit of Dikili-dagh.

The greenish, somewhat schistose rocks, with sandstones of the same color, near Ahmadja, as west of Smyrna, overlie the limestone. The cretaceous age of the limestone at the locality last named appears to be quite definitely determined by Strickland, Tchihatcheff, and Spratt; but the age of that near Ahmadja is yet uncertain. Only one fossil has been found in it. Concerning this, Professor Neumayr writes, "It is a Rhynconella which is so widely distributed that it cannot be used as a certain means of determining the age of the strata in which it occurs; but the limestone is probably cretaceous."

That these rocks are younger than those of the mica-schist zone is indicated, not only by the fact that they contain fossils, and are less crystalline than that group, but also by the fact that they are made up of sediments derived from the crystalline schists. On the other hand, that they are, at least in part, old rocks, is shown by the contact zone produced in them by the quartz diorite.

In 1881 Mr. Frank Calvert, American consul at Dardanelles, discovered undoubtedly eocene fossils (determined by Professor Neumayr) at several places in the Troadic peninsula outside of the region visited by the geologist of the expedition. The same rocks, in all probability, occur also in the southern Troad; but, until further investigations are made, their appearance must be left doubtful.

It seems probable, therefore, that in the intermediate zone there are a number of terranes of different age. It should be stated in this connection, that the rocks of the southern Troad, placed by Tchihatcheff provisionally in the lower tertiary, are, according to Professor Neumayr, of more recent origin.

The third or youngest group of stratified deposits, embracing those which are certainly not older than the miocene, may be divided into two portions. Geographically they are entirely distinct, and their stratigraphical relations are yet uncertain.

The rocks of the sarmatic stage (tufa) of the miocene, so well exposed at Eren-kieui, are now known to border the western coast from the Trojan plain to beyond the mouth of the Touzla, near the promontory of Baba-bournou.

At the site of ancient Hamaxitos, several kilometres south-west of Kinlahly, the 'mactrakalk,' with its characteristic fossils, forms the acropolis. This limestone is undoubtedly of marine origin; and although it has a wide distribution north-eastward, toward the Caspian and the Vienna basin, yet it has not been recognized farther south-west than the coast of the Troad.

Beneath the limestone, as at Eren-kieui, is a great thickness of sand and clay beds which are underlaid by a conglomerate, and probably at the bottom of the series a stratum of red clay. The conglomerate is composed chiefly of fragments of andesite and liparite. Fossils have not been found in these beds near Hemaxitos; but at Eren-kieui, according to Calvert and Neumayr, organic remains are not infrequent, and of a mixed character, indicating that the strata belong, at least in great part, to the sarmatic stage. The marine beds which overlie the mactra limestone are largely developed south of the mouth of the Touzla, and contain great numbers of fossils, among which are many Ostrea and gastropods.

The second portion of the tertiary deposits occupies a large part of the interior of the Troad about the great plain of the Menderè, between Ezine and Bairamitch, as well as along the southern coast, west of Papazlỹ. It has furnished but few fossils, and they are of such a character that its age cannot be determined with certainty. However, according to Professor Neumayr, who has kindly undertaken the determination of the fossils collected by the expedition, it must be upper miocene, miopliocene, or lower pliocene. That it is in great part a fresh, or at most a slightly brackish water deposit, cannot be doubted. As has already been shown in a preliminary report, where these deposits are described at some length, the basis of the series is a conglomerate in which fragments of the basalts, andesites, and liparites, have not been found. It is overlaid by a series of shales, upon which, between Demirdji-kieui and Narly, rests a puzzling rock, regarded by Tchihatcheff as limestone. It is usually pale-yellowish colored, soft, light, and

porous, and generally shows no trace of effervescence in hydrochloric acid. In general appearance it closely resembles an impure siliceous limestone from which the greater portion of the carbonate of lime has been leached away. Having a thickness of about a hundred and thirty metres, it becomes the chief topographical determinant of that region, and gives rise to profound gorges and bold escarpments. Throughout the greater portion of the mass, it is uniformly fine-grained, but under the microscope has the structure of a tufa.

The upper beds of the series, consisting of thin fresh-water limestones, sandstones, shales, and a large proportion of stratified tufas, with conglomerates, have not been seen east of Demirdji-kiœui. The fossils collected were found in this portion of the series; and it is evident that the ejection of the andesites began before the deposition of those beds was completed.

Numerous oscillations of the land, as indicated by the varying character of the strata, must have occurred during the miocene and pliocene; and, in all probability, these were connected with the extrusion of the eruptive rocks so abundant in that region.

The massive rocks of the Troad belong in part to those of pre-tertiary origin, but the greater portion were extruded since the beginning of the tertiary period. The older group includes biotite-hornblende-granite, quartz-porphyry, quartz-diorite, augite-porphyrite, melaphyre, and serpentine, while the younger group embraces liparites, andesites, augite-andesites, basalts, and nepheline-basalt.

The biotite-hornblende-granite occurs in a stock-like mass, forming the serrated ridge of Chigri-dagh. It is distinctly younger than the highly crystalline stratified rocks which it penetrates, and is especially interesting from the fact, that, where it is altered, the titanite is changed to anatase. The alteration of titanite and ilmenite to anatase is doubtless a common and widely distributed occurrence; but, as the crystals of anatase are so small, they have generally been overlooked.

The quartz-porphyries are chiefly microgranites, and are younger than the biotite-hornblende-granite through which they have been extruded. The dikes in which they occur are comparatively small, and do not exercise much influence upon the topographical features of the country.

The quartz-diorites form a number of comparatively small stöcke about the base of Mount Ida, and are evidently younger than the quartzose argillite of the middle zone of stratified rocks, which, in one case, has been metamorphosed into a cordierite and andalusite hornfels. It is to be especially noted that these eruptive rocks do not, as formerly supposed, enter into the structure of Mount Ida.

The augite-porphyrites (diabase-porphyrites) and melaphyres are, as far as yet known, limited to five outcrops, all lying in a line near the southern coast of the Troad, and, with the exception of that between Ahmadja and Qyalar, are not important. At the locality just named it is of especial interest from the fact that melaphyre was the first rock extruded in that isolated (completely surrounded by tertiary strata) volcanic centre, and was followed later by mica-andesite, hornblende-andesite, augite-andesite, basalt, and, late if not last, by a large outpouring of liparite.

The serpentine in the anterior part of the Troad about Qarà-dagh has been derived from olivine-enstatite rocks of a truly eruptive nature. The almost entire absence of the characteristic reticulated structure in some of the serpentine from the Kemar valley leaves, perhaps, some doubt as to the original rock from which it has been derived. As previously stated, the serpentine about the summit of Mount Ida has been derived from olivine-schist which undoubtedly belongs to the stratified rocks.

Although the ancient eruptive rocks are apparently not nearly so abundant as those of more recent origin, yet they represent very nearly the same range in chemical and mineralogical composition. The granite and quartz porphyries have their modern equivalents in the liparites; the quartz-diorites, in the mica and hornblende andesites; the augite-porphyrites, in the augite-andesites; the melaphyre, in the basalt. However, no equivalents were found for the nepheline-basalts and the ancient olivine-enstatite rocks. On the other hand, the syenites, and their modern representatives the trachytes, which were once supposed to be abundant in the Troad, are now known to be at most only very sparingly represented.

The liparites occur in various types, with many varieties, and are limited to the southern part of the Troad. They appear also south of Molivo on the island of Mitylene, and at Sal Mosac south-west of Aivalŷ. They are generally in the stony condition, but frequently glassy upon the boundaries, and contain many fragments of the andesites which they have penetrated and overflowed. They always occur in dikes, as at Qozlou-dagh and the great plateau, which give rise to the peculiar drainage of the Touzla River. That some of the liparites

were extruded before the deposition of the 'mactrakalk' is certain; but, from the fact that the exact age of the tertiary deposits in the southern part of the Troad has not been definitely determined, the time of the extrusion of the great mass of the liparites cannot be stated. However, it occurred most likely at the beginning or in the early part of the pliocene, when the land was raised above the sea, and the islands converted into a peninsula.

The andesites embrace typical mica-andesites and hornblende-andesites, as well as a great variety in which mica and hornblende occur in nearly equal proportion. These, with augite-andesite, occupy a great area between the Menderè and the southern coast; and, unlike the liparites, they seem to have reached the surface, at least in some cases, through volcanic vents. Not unfrequently they occur in dikes also, and have evidently overflowed a large area of late tertiary deposits.

Their extrusion along the western coast began before the deposition of the 'mactrakalk,' and along the southern coast during the formation of the fresh-water deposits of that region. Pyroxene is generally a prominent constituent of the andesites, and frequently both rhombic and monoclinic pyroxenes occur together. The former is generally the most abundant, and has in one case been proved to be hypersthene. It occurs not only in the mica-andesite at Assos and Smyrna, but also in the hornblende-andesite north-west of Qozlou-dagh, and the augite-andesite west of Sivriji-bournou. Among the great variety of andesites may be mentioned the oldest which flowed from the crater at Assos. It is a mica-andesite, in the groundmass of which is a large proportion of apparently primary mica and hematite.

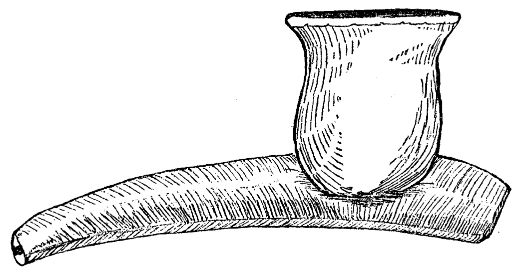
The basalts occur in dikes, and, although widely distributed, do not occupy large areas. Along the southern coast of the Troad it is of an andesitic type, and the olivine is occasionally altered to distinctly cleavable pleochroitic serpentine.

The same phenomenon is better developed in the typical nepheline-basalt which forms the prominent hill called Qaràlyly or Qapandjàtepe, near the centre of the Troadic peninsula. The basalts and nepheline-basalt are evidently younger than the tertiary deposits with which they are associated; but the time of their extrusion with reference to that of the other eruptive rocks of the Troad cannot be definitely determined. J. S. DILLER.

Greason, Cumberland County, Penn.,
June 4, 1883.

OCCURRENCE OF MOUND-BUILDERS' PIPES IN NEW JERSEY.

UNTIL recently the one form of stone implement which is characteristic of the mounds of Ohio and westward, and that has not been duplicated in surface finds in New Jersey and elsewhere on our northern Atlantic sea-board, is the so-called mound-builders' pipes, such as were discovered in great numbers, and described in detail by Squier and Davis in the 'Ancient monuments of the Mississippi Valley,' and more recently by several authors. These pipes may be characterized as having a small bowl, usually in the shape of a bird, mammal, or human head, placed upon a short, flat, and slightly curved base, so perforated that it was used as the stem of the pipe. In other words, it was a complete smoking implement, and therefore unlike the ordinary pipes or pipe-bowls found in New Jersey and the New-England states, which, as a rule, required the addition of a stem of reed or hollow bone, to be used as the mouthpiece.



Within a few weeks, a pipe of the pattern I have described, assumed to be peculiar to the mound-builders, has been found in New Jersey. While the bowl is perfectly plain, except a slight scalloping of the rim, it will be seen at a glance, that the specimen is essentially of the same pattern as the 'animal pipes' found in Ohio, and recently also in Iowa.

Previous to 1882, I had been unable to find any pipes of this pattern, or traces of native copper implements of any kind; but since then copper spears, such as are found in Wisconsin, have been found in New Jersey, and now the pipe that I have described, and of which an illustration is given. Recently, also, specimens of flint arrow-heads have been collected, which in size, and delicacy of finish, are equal to the best examples from Oregon.

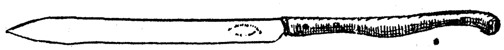
These specimens are now briefly referred to, as indicative of the fact, that in skill in working flint, and in the range of handiwork, whether in stone, bone, or clay, the difference

between those people that erected the extensive earth-works of the Ohio valley and elsewhere, and the 'wild tribes' of the Atlantic seaboard, is practically nothing. I still hope to find unmistakable artificial mounds in New Jersey; basing my expectation upon the fact, that natural hillocks or knolls were frequently used as places of burial, and were chosen as desirable sites for the erection of wigwams.

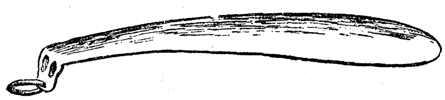
Charles C. Abbott, M.D.

THE IGLOO OF THE INNUIT.[1]—III.

The only instrument used in the construction of the igloo is the snow-knife. Where the Innuits have intercourse with white men, they barter for cheese-knives or long-bladed butcher-knives, remove the double handle from the tang, and put on a single one about three times as long, which can be readily grasped by both hands. The old knives were made of reindeer-horn or from the shin-bone of the reindeer.



MODERN SNOW-KNIFE.



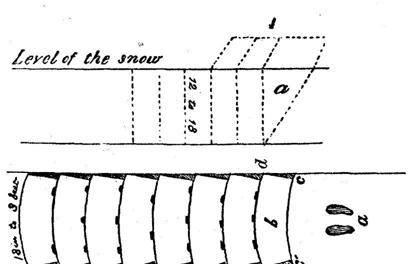
SNOW-KNIFE OF BONE.

Among the Esquimaux in and around King William's Land I found snow-knives made of copper stripped from Sir John Franklin's ships, the imprints of the queen's broad arrow still showing on many, the blades double-edged or dagger-shape, and the handles of musk-ox and reindeer horn rudely attached by sinew lashings.

The snow-knife of iron, while more convenient in many ways, is far more liable to break in the intense cold of the winter weather, such accidents with them being very common. I have seen igloos built when the thermometer registered −70° F. At such temperatures the snow becomes almost stone-like in its compactness. The snow-knife is often used as a substitute for the snow-tester whenever that instrument is broken or left behind, for the Esquimaux are a very careless and absent-minded people.

Before starting to cut the snow-blocks, the builder gets from the sledge a pair of gauntlets used for this purpose, only being of finer and softer reindeer-fur, so as to give the hands the most complete freedom of motion. These gloves extend half way up the fore-arm, and have a puckering-string around the top, which the builder's wife pulls tight, and ties so as to completely exclude the snow while he is at work in it.

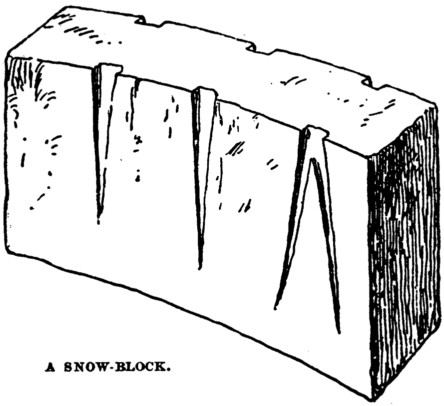
The igloo is built on the sloping drift of snow, the entrance being at the lowest point. The first trench from which the snow-blocks are cut is so disposed as to have its axis coincident with the diameter of the igloo, which runs directly up and down hill, or which makes the greatest angle with the horizontal. These snow-blocks are from a foot to a foot and a half wide, from a foot and a half to two or three feet long, and eight or ten inches thick. The first block cut from the trench is a thick triangular one, which is thrown away (see *a*, which is a vertical section through the axis of the trench). A ground plan of the blocks would show that they are partially curved, but in no manner to such an extent as would be needed to conform to the curvature of the igloo. This curvature is the result of their manner of cutting by a swinging motion of the whole body, held almost rigid, and rotating about the foot steps, *a*, in the figure. This motion of the whole body gives them considerable power; and the resulting curved blocks, if large, are in the best shape for the first part of the structure. In cutting the block *b*, first the right-hand edge, *cd*, is cut by three or four powerful downward strokes of the knife, and then the opposite edge, *c′d′*. The knife, with its blade held horizontally, is passed under the block in front of the toes of the builder's feet. About three or four inches in depth of the line *d′d* is cut; and,



with the knife in the right hand, two or three deep vertical thrusts are made along this line, which generally separate the snow-block from its bed, and it is caught with the left hand as it falls forward. I have tried to represent these gashes in the figure. They are plainly visible on the snow-block inside and out, and a good artist would represent them in his pictures of the huts. The blocks are carefully lifted out and placed beside the trench, as, under some circum-

[1] Continued from No. 29.

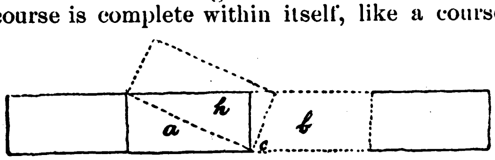
stances, they are extremely liable to break in handling. If the snow has been properly tested,



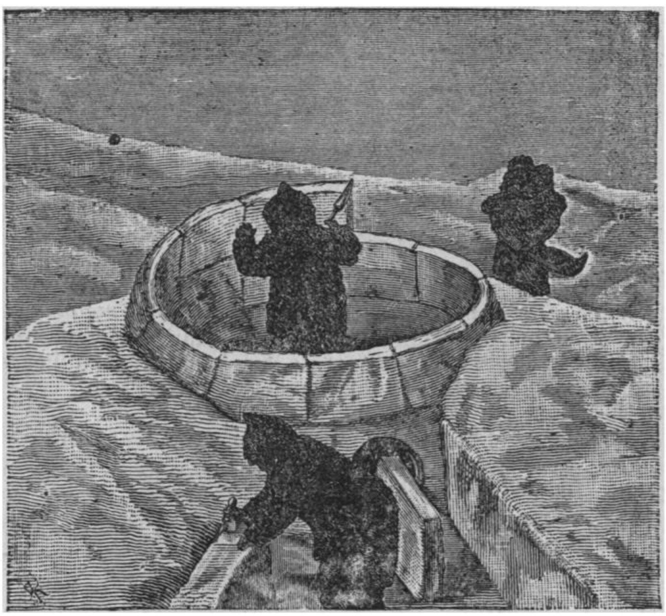
A SNOW-BLOCK.

this should, however, seldom occur. The trench completed, and enough blocks secured to form the first or base course, the floor is laid out by a circular sweep of the knife, varying in diameter, of course, according to the number of intended occupants. Commencing at the left hand, this course is laid until the first block, *a*, is reached, which is cut in halves from its first lower corner, *c*, along the ascending diagonal; and the top half, *h*, is thrown away. The last block, *b*, has its contiguous corner cut off; so that the next block, shown in broken outline, ascends and forms the first block of the next course. The igloo is then formed of this spiral of snow-blocks, each course inclining inward slightly more than the one previous, until the last, which may be called the key-block, is perfectly horizontal, and firmly wedges in and binds the whole structure. This spiral form of the courses I have tried to show in the illustration of one of the half-completed igloos.

I know that the general idea is, that each course is complete within itself, like a course



of bricks on a round tower in our method of building; but a moment's thought would show this to be almost impossible, as the first block in the course, after they had commenced to lean considerably, would have to be supported until it was flanked by others; and these, again, would be very unstable. In fact, one often wonders how a snow-block will hold in place against its own weight, leaning far inwards, almost horizontal, and supported only on two sides, and will imagine that the native workmanship must be very good to give such results. As the blocks approach the top, — where they are more nearly horizontal and more liable to tumble down, — their figure becomes trapezoidal in order to keep the vertical joints pointing to the centre and top; and, while supported on but two sides, these form a more or less acute angle, — more acute as it is needed and approaches the top, where the last few blocks are made triangular and meet at a point. The workman stands inside until it is completed. Despite all the care, the falling of



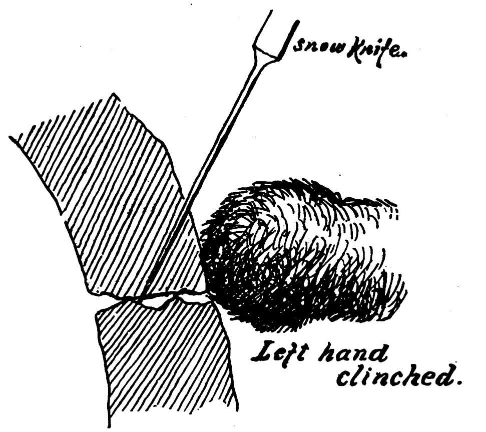
THE HALF-BUILT IGLOO.

blocks is a very common occurrence, and happens with nearly every building.

It will be remembered that the base course has been laid upon a sloping bank of snow, the lowest point being at the door, which has been formed by the trench running into the building. Therefore, when the builder is coming down with a course of blocks on the left side, they are peculiarly prone to tumble in. The fact that this side is used for starting up on the

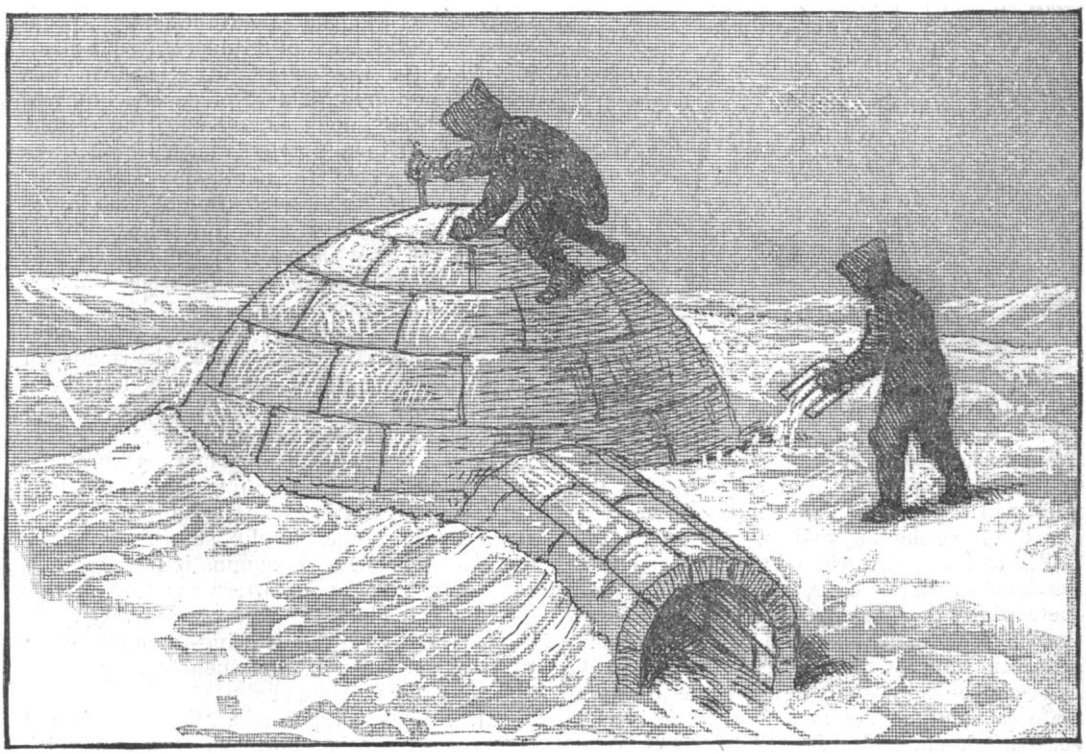
spiral course, as already explained, assists somewhat to overcome this; but it is mostly remedied by the builder, as each round is made, trimming down the up-hill part of the course to about half, until, by the time the blocks are leaning considerably, the course is level (leaving out the spiral inclination).

As each block is being fitted, it is held near its intended position by the left hand of the builder, who at one stroke cuts off the triangle on the right edge, giving a trapezoidal form. The left edge of the preceding block receives the same treatment, and the block is shoved into place. The snow-knife is rapidly passed backwards and forwards in the joints at the side and bottom, cutting off all inequalities, and making a fine powdery snow, which acts as a binding mortar. The last act is to give the block a sharp shoving blow with the open hand from the top, and another from the left side, which firmly sets it in place. The blocks all laid, the igloo is now complete, except the 'chinking' of the joints to render it air-tight, there being many large crevices. The chinking of an igloo is a very ingenious affair; the material being cut diagonally from the lower edge of the upper block on the horizontal joints, and from the left edge of the right block on the vertical ones, if the person be



VERTICAL CROSS-SECTION THROUGH WALL OF IGLOO.

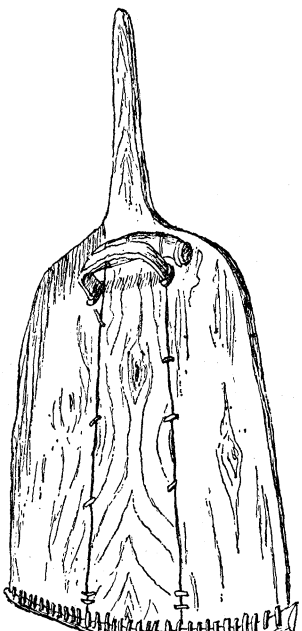
right-handed. As the knife in the right hand thus trims the edges, the left fist, tightly clinched, follows the knife, and rams the cut portion tightly into the crevice, rendering it



THE FINISHING TOUCHES.

as perfectly air-tight as the body of the snow-block itself. An active Innuit will go completely around the igloo on a single joint in about a minute, and it seldom takes over ten to do all the chinking in a large hut. This part is generally assigned to the boys and women, especially the former, who are much lighter, as it is necessary to go on top to complete their work. A well-built igloo, however, will readily bear the weight of two large men on their hands and knees; and yet I have seen a small boy fall through one made of friable snow.

Meanwhile the boys and women have been busy throwing the loose snow from the trench-



THE SNOW-SHOVEL.

es, and piling it on the house, often following closely upon the work of block-laying, covering the whole to a depth of from six inches to half as many feet. The depth to which this is carried depends on the length of time they expect to use the hut, and on the temperature.

The common pictures of the huts, showing the block-work so conspicuously, are largely the work of the imagination of the artists, all that is seen being rounded heaps of rough granular snow. Such artistic license may, however, be allowable to show the essential features; and, so far as my criticism is concerned, I do not wish to be understood as saying that such uncovered igloos never occur.

I have spoken of the snow-walls, when chinked, as being perfectly air-tight. This is not strictly correct; the snow being more or less porous, and allowing a slow but ample current of air to pass through. In fact, at night the door is sealed, and the only means of ventilation is through the body of the snow.

In 1879, during a heavy north-east gale, I was in an igloo on the west bank of Back's River. The walls were of a granular snow, but were covered to a depth of three or four feet. Yet, with all this thickness, a candle-flame held near the wall on the windward side was deflected constantly at an angle of from thirty to thirty-five degrees from the vertical.

The banking is done with a snow-shovel made of half-inch boards, tapering off to a short handle for one hand: a bent piece of musk-ox horn fastened in at the centre furnishes a hold for the other. The cutting edge is protected by a sharpened shoe of reindeer-horn, neatly bound on with reindeer sinew, which is also used to sew the boards together. The Netschilluks use shovels of cedar, walnut, and mahogany from Franklin's ships.

(To be continued.)

MINNESOTA WEATHER.

MUCH has been said about the sanitary properties of the climate of Minnesota as a healing-place for the consumptive; and in this connection a great deal of erroneous information has been published, often to the serious injury of the invalid, who is misled by it. As might be expected, the newspaper is the principal agent in the dissemination of such literature. Here is an extract from the editorial page of the St. Paul and Minneapolis *Pioneer press*, the leading journal between Chicago and San Francisco: —

"Of the aid that may be given by a pure, rarefied, and dry atmosphere, thousands of people now living in Minnesota, who have been rescued from impending death, can bear substantial and grateful testimony."

Written in the haste of a newspaper office, by one who is practically pledged to the laudation of his state, as the western editor is, such a paragraph would scarcely deserve notice, were it not a summation in brief of some of the most popular errors afloat on this subject, and which one meets with everywhere in that land, from the drawing-room gossip to the medical journal. As such, it may profitably serve as text for analysis.

In the matter of pure air, Minnesota is not different from other northern states in which the face of nature has been moiled by the

hand and habitation of man. On the prairies and in the pine-woods the atmosphere yet retains a large share of its pristine purity: in the cities it is the reverse. Especially is it vitiated in the large and rapidly growing cities of St. Paul and Minneapolis, whose systems of water-supply, drainage, garbage-removal, and sanitary inspection, cannot keep pace with their increase of population. This fault will be remedied in time, however, when the authorities shall have learned that the doubling or trebling of a city's people in a decade brings with it new responsibilities as well as new prosperity. It is an easy and pleasant thing to boast that one's town is gaining population at the rate of a thousand a month, and that the values of real estate are rising accordingly; but the real-estate owner is slow to appreciate the necessity of advancing the salaries of city officials, and the appropriations for city improvements, with corresponding alacrity. Minneapolis, although built upon the flat surface of the prairie, has admirable opportunities for drainage into the adjoining gorge of the Mississippi River; but its dilatoriness in this and other works of sanitary improvement has been severely punished by the scourge of typhoid-fever. The prevalence of this disease has caused Minneapolis at times to stand at the head of the column of death-rates of the cities of the United States. While there may be malaria in Minnesota, — and, indeed, the term is sometimes found in the reports of the physicians, — it is by no means the popular disease that it is in the south and east, where it is almost the fashion. A person may spend a year there without hearing the word mentioned; and that immunity alone should be enough to stimulate emigration in that direction.

Dryness of atmosphere is claimed for Minnesota; and if we consult only the amount of rainfall, whose annual value ranges from twenty to forty inches, there is apparent justice in this claim. But the manner as well as the amount of the pluvial precipitation must be considered. They have in that state a good deal of the lachrymose English weather, in which a drizzling dampness takes the place of the short, sharp, and decisive showers of equatorial lands. At the close of a rainy day the observer will go to his rain-gauge, and find its bottom scarcely covered. The effect of effort without accomplishment is always a depression of spirits in the looker-on; and this rule is never truer than when applied to a rainy day. Those who spent the month of October, 1881, in Minnesota, will remember it as a season of almost continual storm, during which, even when there was no absolute rainfall, there was an unwholesome mist floating in the air. Occasionally the sun shone, but not with sufficient power to make an impression. Farm-labor was almost suspended. The potatoes rotted in the ground, and the wheat grew in the stack. The streets of Venice were scarcely more liquid than the streets of St. Paul. Danger-signals were erected in the fashionable avenues to warn teamsters away from fathomless depths of mud. Hackney-coaches were stalled there, and their horses were detached, leaving the vehicles to be extracted by the processes of engineering. So impassable were the roads, that the fuel-supply was unequal to the demand, and invalids were obliged to go to bed to keep warm, and public schools were closed because their pupils were frozen out.

Still the rainfall of this month was less than four inches and a half. Many a single shower in the warm latitudes precipitates an equal amount of water. Indeed, there are records of rains in which as much water has fallen in one day as falls in Minnesota during the year; but, as a light rainfall does not necessarily mean a dry atmosphere, neither does an excessive precipitation invariably make a wet one. The water may flow away quickly, leaving no sign; and the next day the sun may shine as brightly as ever. Better, therefore, for the lungs, is an occasional drenching than a perpetual drizzle. While it must be admitted that the weather of the October just quoted, although not so bad as that of the September preceding, was yet exceptional in the extreme, still such exceptions could hardly occur in a very dry climate.

The student of physical geography would scarcely expect to find the climate of Minnesota a dry one. An average of such statistics as the writer has at hand indicates that rain or snow falls at least every third day in St. Paul. The state is almost directly under the influence of the Great Lakes, and is itself threaded with rivers, and dotted with lakes. Of the latter there are eight thousand worthy of the name, besides innumerable ponds. Two large river-systems receive their waters from the drainage of this region. The swamp-lands of the state play an important part in its area, as the maps of the land-office show. A large share of its forests are afloat upon ancient marshes. Cranberries and rheumatism abound. The Red River region is celebrated for its floods. At one time that stream was popularly said to be thirty miles wide; and

the traveller down its valley was obliged to proceed by alternate stages of land and water, the steamboat being utilized when the railway-cars began to swim. Then it was that the facetious pilgrim from St. Paul to Winnipeg was, according to his habitual description of the journey, three days out of sight of land. It was a joke, to be sure; but such jokes are not heard in a dry climate.

The moisture of the atmosphere of Minnesota is the salvation of the state: it makes agriculture a possibility and a success. Given the same amount of rainfall in another latitude, and under more arid climatic conditions, and her wheat-fields would be blighted. As it is, her scanty rains, with the exception of a few showers in summer, fall slowly and gently; in times of drought the thirsty air freights itself with moisture from the abundant water-surface of the state; and these sources of humidity are re-enforced by the prolonged irrigation resulting from the melting of the winter snows and the thawing of the frozen ground in spring.

The beneficial effects of an unclouded sun in the treatment of consumption may, perhaps, be overrated. The dweller in a rainless atmosphere, dazzled by the perpetual brightness, and with lungs parched by the heat and dust and dryness of the air, might come at last to long for an occasional rainy day, as the traveller in the desert longs for the shadow of the palm. But, at any rate, our weather bureau could scarcely do better work than to give us a 'sunshine map,' upon which the statistics of hourly observations the year round, upon the state of the sky, should be graphically portrayed. Such frequent observations could be taken without inconvenience, as it would not be necessary for the observer to remain at a fixed station for that purpose. Such a map would show by depths of shading the relative amounts of sunshine and cloud at any place; and the invalid could select at a glance a residence which would have the desired proportion of these conditions. The complexion of Minnesota upon such a map would probably not vary widely from the average.

As has been seen, there is also a popular belief that the air of Minnesota is in a very rarefied condition. In the interests of meteorology, that superstition must be met and combated. The only cause of rarefaction of atmosphere worth considering here is elevation above the sea. Minnesota, as one might guess from its position in the Mississippi valley, is a low country. The mean elevation of the United States above sea-level is about twenty-five hundred feet. The average elevation of Minnesota is considerably less than half that number. Indeed, its 'height of land' falls much below twenty-five hundred feet. Therefore a large proportion of visitors to that state move into a heavier atmosphere than that which they have left; but unfortunately they do not know that fact, and, under the influence of their imaginations, they find their breath wonderfully shortened. The elevation of St. Paul above the sea is seven hundred or eight hundred feet; that of the plateau region of New York is from a thousand to two thousand feet. I once knew a lady to remove from the latter to the former place, thus going down hill and into a denser atmosphere. Arriving in St. Paul, she could with difficulty climb a flight of stairs, owing to the lightness of the air, as she expressed it. When informed of her mistake, she was indignant, and resented the information. People do not like to give up their errors, even if they are uncomfortable ones. Having come a thousand miles in search of novelty, it was strange and cruel if she could not be allowed to enjoy that novelty which is supposed to be characteristic of the west, — a rarefied atmosphere. With all its benefits, science works mankind an occasional mischief. The mountaineers of old suffered no inconvenience from their exalted position until the meteorologist came along, and explained to them that the air grew constantly thinner as they approached the clouds. Even to-day the unlearned inhabitants of our Rocky Mountain region make no complaints of a difficult respiration. It is only the scientific tourists who pant by the aneroid, and cough up a little blood when they cross the timber-line. Whether appreciated or not, however, it is certain that the air of the uplands is less substantial food for the lungs than that of the low countries; and it is the density of the atmosphere, and not the reverse, which is to the advantage of Minnesota as a home for the consumptive. There are many people who advise this unfortunate to seek out some elevated region in which to live, but there are very few who can give any reason for this counsel. A learned doctor tells us in one of the late magazines, that the harmful substance known as carbonic-acid gas is more abundant near the level of the sea. Certainly; since there is more air to the cubic measure at a low elevation, there is naturally more carbonic acid, which exists in the atmosphere, whether high or low, in a certain percentage of the whole; but there is at the same time more of the saving grace of oxygen, which

the invalid is after. It is true that carbonic acid has a way of accumulating in low and unventilated recesses; but there are cellars, crevices, and deep and narrow valleys in the highlands as well as on the lower levels. As well recommend thin soup to the hungry man as to advise the sick man, whose one lung must do the duty of two, to breathe thin air. Should he climb the mountains to Leadville, he will be warned away by the inhabitants of that city, who will inform him, in the rude poetry of the mines, that a healthy man has to fan the air up into a corner in order to get enough for a breath.

The atmosphere is not necessarily dry at a great altitude, as some suppose, nor damp in the lowlands. There are lofty swamps and low deserts. The mountain peaks, according to the poet, milk the clouds; and in some parts of the world the mountaineer is more sure of his daily rain than of his daily bread. Mount Taylor, in New Mexico, is called the 'Mother of rain' by the imaginative Indians. On the other hand, the deserts of California, which are below the level of the sea, are so dry, that, in the language of the plains, the jack-rabbit has to pack his water with him when he goes upon a journey.

As to the thousands who have been rescued from death by the 'pure, rarefied, and dry atmosphere' of Minnesota, this is a matter of town talk, which impartial observation does not confirm, and which there is no census to deny. In this connection I would challenge the champion of the most celebrated sanitarium for consumptives to produce a list of the patients who have 'got better' under his notice, and I will match against him an equally honest observer from some undistinguished and unpretentious and confessedly unhealthy locality, whose proportional record of improvements will be equally favorable. Why, then, should the sick man become a wanderer, as he certainly will if he once starts in chase of the *ignis fatuus* of a climate cure?

Frank D. Y. Carpenter.

LETTERS TO THE EDITOR.

Prehensile feet of the crows.

In nos. 16, 18, and 20 of Science are communications by different writers on the intelligence of crows, suggested by one of mine in no. 13. I beg to add one more, concluding what I have to say on this subject.

All seem agreed as to the intelligence of these birds; but few, I find on inquiry, have seen them seize or carry objects in their claws. Yet no amount of negative testimony should invalidate my observation on the Italian bird, when taken in connection with the further evidence to be given. We all look at nature piecemeal; and it is certainly unreasonable to assume that one is in error because he claims to have seen through his pin-hole something which another has not observed through his.

I agree with the doubters, that crows ordinarily use their bills, and not their claws, in seizing and carrying their food. In confirmation of what I claim to have seen, I will adduce similar instances, noticed by others as well as myself, in the Corvidae. I cannot positively assert that the bird I saw was C. corone: it might have been C. cornix, possibly C. frugilegus, but, at any rate, a *crow*, for it had the flight, the proportions, the color, the voice, and the boldness of these birds.

As to crows not nesting among rocks, this is generally true of the American crow (C. Americanus); but the European C. corone, a larger and more solitary species, prefers the sides of steep rocks, as also does the hooded C. cornix. Both the American and European ravens often nest in inaccessible cliffs, and so do the rooks.

To begin with the largest. I have seen C. corax in Iceland holding and carrying in its claws fish-heads from the beaches, and, when disturbed, from one barren crag to another, — an object too large and too heavy to be conveniently carried in the bill, and too precious to be left behind where food is so scarce. I have seen C. carnivorus, in the winter wilderness of Lake Superior, carrying in the same way what looked like a squirrel or rabbit. It is well known that both these birds, when wounded, will strike savagely with their claws, like a bird of prey; which, being perching birds, according to our classifications they had no scientific right to do.

Of the fish-crow (C. ossifragus), Wilson (*Amer. ornith.*, v. 27) writes, "their favorite haunts being about the banks of the river, along which they usually sailed, dextrously snatching up *with their claws* [the italics are mine] dead fish or other garbage that floated on the surface;" and, on p. 28 (*op. cit.*), "These (a singular kind of lizard) the crow would frequently seize *with his claws*, as he flew along the surface, and retire to the summit of a dead tree to enjoy his repast." Audubon (*Orn. biog.*, ii. 269) says the same. Clark's Columbian crow is said to do the same thing, and its claws are sharp and raptorial. I have seen this species, along the shallows of the coast of North Carolina, seize and carry off in its *claws* living fish from the shoals over which it flew.

Buffon, Chenu, Wilson, and Nuttall allude to the custom of capturing crows by fastening one on its back, feet upward, on the ground: its cries bring its companions to the rescue, one of whom is sure to be seized and held by the *claws* of the prisoner.

For several summers I lived in the next house to a tame and speaking crow, which often came in front of the kitchen in quest of food. One day a half-eaten ear of boiled corn was thrown to him. While engaged in picking it, holding it by the claws, as is the habit with the crows, he was disturbed by the attacks of a barking terrier. Keeping him at bay for a time by vigorous pecks, he finally tried to carry the ear in his *bill* to a favorite perch in a low cedar. As he seized it, first at one end and then at another, the leverage of the free end was such that it gave his head and neck very uncomfortable twists. He finally perched upon the ear in defence of his food, and, clinching it tightly in his *claws*, flew with it, in my sight, to his perch a few feet distant.

Mr. E. A. Samuels (author of the 'Birds of New England') writes to me (Aug. 2, 1883), "I have known of its seizing with one foot — and hopping

with the other — various small articles of food, in one case a small frog;" and also, "I have often seen the crow hold a frog or acorn firmly, with one foot on the ground or on a fence-rail, while he pecked away with his bill." Similar instances I remember to have read about, and one in the Bulletin of the Nuttall ornithological club, where it is described as holding a small bird, which it had killed in an aviary, in its claws, while it tore it in pieces with its bill, like a bird of prey.

The claws of the shrikes, weaker than those of the crows, and quite as insessorial, are used to seize and carry prey. A few winters ago I saw a shrike killed on the Boston public garden by the city forester's men, which had in its claws, during its flight, a still living English sparrow. That the crows in the above-mentioned instances, though perching birds, do use their claws as prehensile organs, I regard as evidence of their intelligence and reasoning power, which enable them, under exceptional circumstances, to use their perching feet for raptorial purposes. We must not measure animal intelligence by our imperfect and arbitrary zoölogical classifications. Since the writings of F. Cuvier, Flourens, and Fée, it seems impossible to deny the possession of a reasoning intelligence to animals below man.

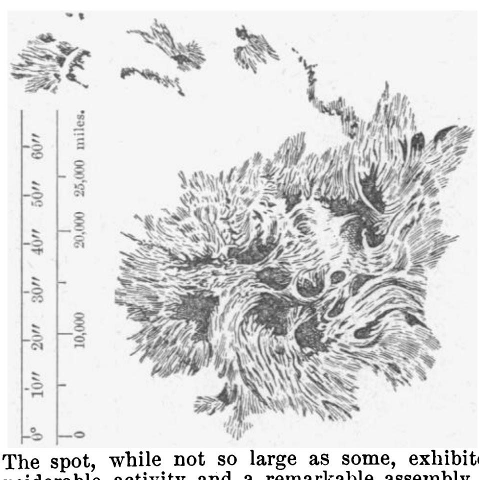
Leaving out of view the instance mentioned in no. 13, I think I have adduced sufficient evidence that the crows do *sometimes* — that is, when they find it necessary — seize and carry objects in their claws, like birds of prey. SAMUEL KNEELAND.

An interesting sun-spot.

The accompanying sketch represents the remarkable sun-spot of July (which was visible to the naked eye), and is of particular interest. I did not see it in its early or formative period, when this was taken; but from my knowledge of Mr. Very's experience and skill I have no doubt of the trustworthiness of the drawing in all its details. His remarks supply all the further information needed. S. P. LANGLEY.

Cambridge, Aug. 21, 1883.

I enclose a sketch of a large and unusually interesting sun-spot, as it appeared through the great equatorial of the Allegheny observatory, of 13 inches aperture, with the polarizing eye-piece. The drawing was made on the 26th of July, 1883.



The spot, while not so large as some, exhibited considerable activity and a remarkable assembly of odd forms, some of which appear so conflicting that it is difficult to imagine how they can exist side by side. The strong inrush from the following side gave one the idea of a viscid sheet or ribbon, rather than that of a bundle of filaments. It bore a striking resemblance to some of the forms which taffy assumes under the confectioner's manipulation. On the upper or northern side the filaments were more graceful, slender, and grass-like. The southern part was remarkable for the length and intensity of its curved filaments. (The longest could certainly be traced through more than 15,000 miles.) But perhaps the most curious portion was the centre, where a mass, possessed of photospheric brilliancy and fringed with curved and tangled threads, gave one the impression that a recently erupted facula, formed somehow in the very middle of the spot, was being torn to pieces by conflicting currents.

Numerous local whirls were evident, and the south-east half of the spot had a decidedly cyclonic appearance, the rotation being in an opposite direction to the hands of a watch. (It is to be remembered, that the drawing gives the appearance of a projection, and is therefore the reverse of a view by direct vision.) The north-west half of the spot did not show any such rotational tendency. F. W. VERY.

Allegheny, Aug. 20, 1883.

The right whale of the North Atlantic.

I am sufficiently impressed by the utter absurdity of occupying your valuable pages in discussing non-essentials; yet I am called upon by your critic to clear up two points remaining, both of which in any case hardly deserve serious notice. I will endeavor to close this correspondence by stating the facts.

Referring to Scoresby's pictures of the Greenland whale, I was led to attribute to the first or earlier one another authorship, from seeing in it so much error and exaggeration; and this because I had just read in Scoresby's book the following (Arct. reg., vol. i. p. 447. 1820): "I have confined my engravings, as well as my descriptions, to those animals that have come immediately under my own examination, or have been sketched by persons on whose accuracy and faithfulness I could fully depend; while drawings that I have met with, when the least doubtful, have been altogether rejected."

His second figure being so nearly correct, having evidently been carefully drawn from an entirely different and natural study of the animal, it was easy to assume, that, having first taken at second-hand an ill-considered sketch, he promptly replaced it by a better one. In this view it should not be assumed that we had any thing but the kindliest motives in thus speaking of this most eminent and valued man's work. In Scoresby's 'Arctic regions' (ed. 1820) the second figure of the Greenland whale appears. The caudal region, including the flukes, is entirely redrawn, showing the various elements that make up the beauty of those parts, as the carinae, etc. The other features, unfortunately, are not improved; yet more unfortunate is the fact that the earlier figure, with all its imperfections, has come down to us in most of the more important works.

With reference to the corrections of Scoresby's figures, we may point to an old work in the library of the American museum, which, by the way, is not noticed in Mr. Allen's bibliography; namely, "Histoire des pêches, des découvertes et des établissemens des Hollandois dans les mers du Nord, etc. Par Le C. Bernard DeReste. Tome premier. A Paris, 1801." This is an octavo volume, devoted almost entirely to cetaceans, and has large copper-plate engravings, one of which contains a right whale labelled B. franche, and another the sperm whale.

The former figure is in some respects better than Scoresby's, as to form and proportions; but a most singular treatment has evidently been accorded it. The elements of the figure have been transposed, and the belly made to serve the purpose of back, and *vice versa*. It is evident that the figure was copied from a real model, as the baleen is shown correctly, though it projects in one place outside the mouth.

The remaining point relates to the authorship of the volume on whales in the 'Naturalists' library.' The portion of the titlepage of our edition relating to this point reads as follows: "Mammalia — whales, etc. By Robt. Hamilton, Esq., M.D., F.R.S.E., etc."

We now desire to ask our critic how much remains to justify the serious charges which he has caused to be distributed wide-cast over the scientific world, to more or less inevitable damage to institution and person.

J. B. HOLDER.

If Dr. Holder is satisfied with the way he has met 'the serious charges,' I am quite willing to here rest the matter; failing, as I do, to see that any of them are materially vitiated by his defence, while, amid the obscurity of much irrelevant matter, all of the more important ones are virtually conceded.

In regard to the authorship of the volume on whales in the 'Naturalists' library,' not only have I, as I have said before, examined anonymous copies of the original edition, and found it given as anonymous in bibliographies, but have seen it attributed by contemporary British cetologists to Jardine. The discovery, however, of a copy by Dr. Holder, having Hamilton's name as author on the titlepage, of course settles the question.

J. A. ALLEN.

Achenial hairs of Senecio.

Mr. Jos. F. James does not know of any explanation of the use of the threads which are projected from the hairs on the achenia of most species of Senecio, etc. Before calling on SCIENCE to help him, he might read up his text-books, say Gray's Structural botany, p. 306.

BOTANICULUS.

Kalmia or rhododendron.

In reply to Dr. Abbott, in SCIENCE for Aug. 17, I will call his attention to the fact that the woods of the kalmia and the rhododendron are quite distinct in appearance, and are not likely to be mistaken the one for the other. The kalmia wood is frequently found in commerce, in the form of handles for tools, such as chisels and the like. The wood is of a very light pink, with darker streaks through it resembling cells filled with woody fibre.

The rhododendron wood is destitute of such marking. As to size, I have seen plenty of the kalmia, four and five inches through the butt, in the mountains of Virginia; and have had in my possession sticks, large enough for any such purpose as the Doctor names, from eastern Pennsylvania. The rhododendron is an extremely rare plant in Chester and Delaware counties, Penn., but the kalmia is common.

S. P. SHARPLES.

Boston, Aug. 22.

THE SOCIETY OF MECHANICAL ENGINEERS.

Transactions of the American society of mechanical engineers. Vol. iii. New York, 1882. 350 p. illustr. 8°.

THIS third volume of the transactions of the youngest of the three great societies of engineers in the United States is a well-printed large octavo of over three hundred pages. It contains a list of the officers and members of the society, its rules, the proceedings of the Philadelphia meeting of 1882, and the proceedings at a memorial session in remembrance of Dr. A. L. Holley, a distinguished engineer and a founder of the society. The proceedings at the latter meeting consisted of an introductory address by president R. H. Thurston, in eulogy of the deceased, and a formal tribute to his memory by Mr. J. C. Bayles, the orator appointed by a committee for the occasion. Many members, as well as the appointed orators, paid earnest and eloquent tribute to the great engineer.

Among the more generally interesting and important papers, are those of Professor Egleston, on the appointment of a government commission to test iron, steel, and other metals; G. W. Bond, on the Pratt & Whitney 'standard gauge system;' Professor Robinson, on the thermodynamics of the Worthington pumping-engine; an essay on the progress of engineering science from 1824 to 1882, by Mr. Fraley of the Franklin institute; the windmill as a prime motor, by Mr. Wolff; and a long paper on the several efficiencies of the steam-engine, by Professor R. H. Thurston.

Professor Egleston gives a history of a movement among the engineers and scientific and business men of the country, to secure the establishment of a permanent commission to determine, by direct investigation, the absolute and relative values of constructive materials in the United States. Under the lead of the Society of civil engineers, such a commission was demanded by a very large number of the leading men of the country, and was created by act of Congress in the year 1875. It consisted of Col. Laidley, Gen. Gilmore, Com. Beardslee, Chief-engineer Smith, Dr. A. L. Holley, and Professor Thurston, the latter acting as secretary. This commission, in the course of two years, working amidst many discouragements, did an enormous amount of work; the results of which are published in a report consisting of two large and fully illustrated volumes recently issued from the government press. The commission was not well sustained. Congress refused to continue its appropriations; and it ceased to exist, despite the protest of all the leading technical societies, polytechnic schools, the principal colleges, and such associations as that of the iron and steel makers. The effort is now making, to revive this commission, and to secure the continuance of its work. The publication of the enormous mass of information acquired by the board during the period of

its short life is hoped to give good argument in favor of prompt and liberal action by another congress, in which, it is believed, there may be a sufficient number of intelligent and patriotic members to carry the measure through without regard to politics.

Mr. Bond describes the method adopted by Professor Rogers of Cambridge, and himself, to secure for Messrs. Pratt & Whitney of Hartford a standard system of exact measures for use in creating a basis for gauges to be used in the United States in general machine construction. The comparator built by the firm, under the advice of these gentlemen, is used. Its readings, with its 'B' microscope, are made from divisions measuring 0.000016 inches. The company has now a set of end measures running by sixteenths to four inches, and a complete plant for making them accurately to within the forty-thousandth of an inch, a magnitude which can be detected by an expert workman.

Professor Robinson gives the theory of the peculiar form of pumping-engine known as the Worthington engine. This is a Wolff form of compound engine in its general arrangement, built without fly-wheel and in pairs, and so constructed that each double engine has its valve-motion operated by the opposite machine. He shows that, theoretically, the 'tandem' type of this combination excels all the other possible adjustments of the engine, in its probable efficiency. The efficiency is not modified perceptibly by the ordinary slight variations of the exponent of the expansion curve. Numerical results of the use of the formulas are given in tabular form. The paper is illustrated by engravings of the several forms and parts of these engines.

Dr. Fraley describes the formation, the growth, and the work of the Franklin institute of the state of Pennsylvania. It was organized in 1824, and has been in active operation ever since. It established the first regular drawing-school in the United States, and has kept it in successful operation for fifty years. It has occasionally given exhibitions of domestic manufactures and products, has gathered together a great library, cabinets of materials, models, and machines, and has for many years regularly published a journal devoted to applied science and the arts.

Mr. Wolff gives the results of investigations of the efficiency and power of windmills, and presents a table, calculated in the course of his studies of the subject, of the relations between the pressure and the velocity of the wind at various temperatures, — the first in which the density and temperature of the atmosphere are taken into account.

Professor Thurston occupies nearly fifty pages in the discussion of the several efficiencies of the steam-engine, including the total commercial efficiency. Expressions are given by which to determine the best proportions of steam-boilers for given costs of boiler and fuel, storage, etc. The best area of heating surface per pound of fuel burned on the grate varies as the square root of the quotient of all annual expenses variable with the cost of fuel, reckoned per pound of coal and per square foot of grate, by the sum of all annual expenses per square foot of heating surface and per square foot of grate, the latter being reckoned only so far as they are dependent on the size of boiler. The efficiency of engine is found to be dependent upon both the ratio of expansion, and the method of variation of waste by internal cylinder condensation with the point of cut-off. Tables are given of the probable best points of cut-off in the various standard types of engines, at various pressures of steam; and also of the probable minimum weights of steam and of good coal required by such engines at various best ratios of expansion.

The 'efficiency of capital' is found to be dependent upon similar quantities, as well as upon the costs of fuel, attendance, operation, etc. The theory of the efficiencies of the ideal engine with non-conducting cylinder is given, and both algebraic and graphical methods of solving problems are presented and illustrated. The theory of the efficiencies of real engines is next treated, and the defects of the Rankine system are remedied. The 'general equation of all steam-engine efficiencies' is given, as deduced by Professor Thurston, and a series of problems falling under the general head are treated by the production of the necessary formulas and by a graphical construction involving the use of his newly discovered 'curve of efficiency.' One-half of the paper is devoted to the solution of various important problems arising in the practice of the engineer and previously unsolved. Tables follow giving the results as applicable to the common forms of steam-engine, and showing the enormous differences in economy and in the best ration of expansion, size of engine, etc., produced by the occurrence of cylinder condensation, a form of waste hitherto untreated by writers on thermodynamics and the theory of the steam-engine. He says, "By the use of this, or some more exact method, the art of proportioning the steam-engine can be elevated to the rank of a branch of the science of

engineering; and that part of the science which has hitherto been in a most unsatisfactory condition, as viewed from the standpoint of the engineer engaged in its application, may be found to take a comparatively complete and useful form.''

GEOLOGY OF PHILADELPHIA.

The geology of Philadelphia: a lecture delivered before the Franklin institute, Jan. 12, 1883. By Professor HENRY CARVILL LEWIS. Philadelphia, 1883. 21 p. 8°.

THE author has distributed his pamphlet edition of this important paper, which deserves extended notice, and has placed him in the front rank of the young prosecutors of original research in the field of geology in this country. This memoir, and his previous lecture on the Ice age in Pennsylvania, have the rare merits that they are solid contributions to our knowledge from the first to the last pages; that they are almost exclusively due to the personal labors of the young geologist who brings them in their very complete form before the world; that they are closely and fairly reasoned out, and lucidly expressed. The great societies of the learned which require for membership the production of a work showing important, new, and original researches, have accepted many essays inferior in all these particulars to the subject of this review. To fully appreciate its merit, one must consider how very vague were the notions of geologists (including the large and growing class of Philadelphia geologists) as to our superficial deposits, before its appearance. The great influence of Louis Agassiz, and his theories of universal glaciation, had restricted the number of those who sought to define the action of glaciers in our continental geology, by extending the limits of this action over the tropics. The explanation of any thing obscure by the words 'glacial action' became almost as common as the explanation of any thing difficult in physiology used to be by the words 'lusus naturae.'

It required, therefore, peculiar independence of thought to break loose from these fictive (always the most insurmountable) fetters, and to see the phenomena with one's own eyes. Besides this, it required laborious journeys, patient note-taking, and attentive reading of what others had done, in order to do justice to the subject, and prepare a monograph upon it. All these Professor Lewis has accomplished; and, though much remains to be done, few presented so complete and neat a view of subject as he has.

It will already appear to be the writer's view, that his matter, and his manner of presenting it, have been found admirable, though as to the latter, his system, while supported by a clear style, will necessarily present some difficulties to the superficial reader. He could either have begun from the exterior and older boundaries of his superficial formations, and have proceeded inwards towards the present river Delaware; or he could have adopted his present plan of commencing in the middle with the red gravel, — inverting somewhat the order of the overlying sediments by considering the alluvium next (which is at the top of all), taking next the Trenton gravel (which underlies the latter), and completing the upper part of the column by treating of the Philadelphia brick clay (which belongs between the upland terrace material, first mentioned, and the Trenton gravel), — and then following the column downward through the red, yellow, and Bryn-Mawr gravels, finishing by a short sketch of the underlying rock formations; or he might have proceeded geographically from the newer deposits on the river, outwards to the Bryn-Mawr terrace.

The writer confesses, that, in view of the perfectly consistent theory which Professor Lewis has evolved, it would seem easier to follow the chronological order of the events which this theory comprehends, even though the geographical sequence were somewhat disturbed; but this criticism does not affect the real value of his results.

Those who read this essay as carefully as it deserves will be rewarded by obtaining a very probable history of this portion of our continent during post-tertiary time, with its submergences and elevations and the consequences thereof. It is perhaps to be regretted that Professor Lewis has not treated with the same care the subordinate part of his subject, to which he devotes a few concluding words; that is to say, the 'gneiss,' the 'auroral limestone,' and the 'triassic sandstone.' Thus, he confounds the views of two masters of our American geology in ascribing the gneiss of Philadelphia in the same breath to the Huronian and the Mont Alban.[1]

It is also somewhat vague to say 'the gneiss of the Rocky Mountains of Colorado;' since there are different gneisses belonging to different ages there, some of them probably Mont Alban, some Huronian, and some very likely Laurentian.

Again: it is conceded by most Philadelphia

[1] Compare Dr. T. Sterry Hunt's view, 2d geol. surv. of Penn., vol. E. p. 200.

geologists, that the section of gneiss along the left bank of the Schuylkill in the Park is not a fair representation of the stratigraphy of the measures. The structure here does not agree with that on the other side of the river for long distances within the limits of the Park, nor with that exposed by the cuts made for streets, etc., at short distances back from the river on this bank. Nor is it exact to say that the measures here dip ‘at high angles;’ since with the exception of a few hundred feet north of Lemon Hill, where one dip of 60° occurs, the dips for three miles are usually 30°, and never over 40°.

Under the caption of ‘Primal sandstone,’ it is the perpetuation of an error to call the ‘sagging’ of rocks standing at high angles ‘*creep*.’ This term is employed by glacialists and mining engineers in two senses quite different from that which Professor Lewis intends to convey, and different from each other. Again: ‘hydro-mica slates’ is a contradiction in terms, though not infrequently used. If the rocks are *slates*, they cannot contain hydromicas, except as adventitious components. The last paragraph of this little pamphlet is very neat and well put; but we may be allowed to dissent from Professor Lewis in the statement that the marble of our doorsteps ‘tells of an ocean inhabited by no fishes:’ at least, mine does not tell me what were *not* in the ocean in which it was formed.

The blemishes in the main work are both few and superficial. Thus (p. 9), it is a little too hasty to infer, merely from the absence of shells or organic remains in a brick clay deposited on a gravel, that the water ‘had a temperature too low to support life;’ p. 11, the colors of the red and yellow gravels are not satisfactorily accounted for by the ‘presence of a large body of water;’ there is a slightly subjective trace in the assertion on the same page, that “there is no trace of glacial action in Pennsylvania south of the terminal moraine, *notwithstanding* all statements to the contrary hitherto made by other geologists,” — which is in contrast with the modest style of other parts of the work; p. 14, ‘Bryn Mawr age’ is not a perfectly clear designation for the time or times when the gravels called by this name were being deposited, especially as there are crystalline rocks exposed at Bryn Mawr.

Notwithstanding these trivial faults (as the writer conceives them to be), the memoir will serve not only to teach our young students of geology to reason from these facts, but will live long, if not permanently, in our literature.

June 25, 1883. PERSIFOR FRAZER.

THE IROQUOIS BOOK OF RITES.

The Iroquois book of rites. Edited by HORATIO HALE. Philadelphia, *Brinton*, 1883. (Brinton's Libr. Amer. lit., no ii.) 222 p. 8°.

THOSE who still hold in remembrance the valuable contributions to linguistics made by Mr. Horatio Hale while connected with the ‘Wilkes exploring expedition’ will be pleased to know that from his retirement in Canada he now sends forth this most interesting work. The reputation of the author, added to this fascinating title, will insure its favorable reception not only by ethnologists, but also the reading public. This aboriginal ‘Iroquois Veda,’ which furnishes the title, and which may be considered a remarkable discovery and indisputably of great ethnological value, is presented in its original Mohawk, with the English translation. An introduction of ten chapters precedes the Book of rites. These are devoted to the general history of the Iroquois, their league and its founders, condolence council, clans and classes, laws of the league, historical traditions, and their character, policy, and language. Portions of these chapters are deductions from the book which follows them.

The boundary-line between either folk-lore or myths, and actual history, is always so vague, that, even in the relation of facts, it is no easy task in their details to so discriminate as to keep truth clear from the brilliant coloring of tradition and conjecture. Especially is this the case when an author with inherited literary taste and vivid imagination enters a realm where the temptation to allow them full scope is as great as in the early history of the Iroquois. Accordingly, we find among these chapters, many of which indicate immense research and are of great value both ethnologically and philologically, those (such as the ‘league and its founders’) wherein the characters are portrayed in so exalted a manner that the sceptical reader will be disposed to assign the story of Hiawatha, as given in all its minute details, not to the realm of mythology even, but to that of classic historical romance. Much less will they be willing to accept it as sober Indian history five hundred years behind its present semi-civilized condition. The chapter on the ‘Iroquois language’ may be considered one of the most important, scientifically, of those in the introduction; and it is probably one of the best outlines of their formation and structure ever published in English, concerning any one of the Iroquois dialects. This fact quite throws the doubt on Mr. Hale's statement that no one except Father Cuoq would

be competent to prepare a grammar of these dialects. With due respect for the great erudition of Father Cuoq, whose special studies have been in Algonquin, although a missionary to both tribes, we would say that the materials from which the reverend father prepared both his Lexique and the Iroquois portion of his Langues sauvages are through the courtesy of the Rev. Fathers Antoine and Burtin, of the order Oblat, now in the temporary possession of our Bureau of ethnology at Washington, where, already nearly translated, they will in time be published in connection with the other Iroquois dialects. We allude to the works of that greatest of all Mohawk scholars, the Rev. Father Marcoux. That the rules, the result of so much time and labor, can be clearly and distinctly presented to us in our own tongue, Mr. Hale has exemplified in the few which he presents in this chapter. The 'forms' and 'particles' which he has given are all from the Mohawk dialect, although he follows the example of all the Canadian authors, who dignify one dialect with the title which others contend belongs properly to a group. The examples he gives will many of them not apply to some of the other dialects, more especially to the Onondaga and Tuscarora.

In following too closely the rules of the French missionaries, great discrimination must naturally be exercised.

We do not agree, for example, with Mr. Hale, in the illustration given with his remarks upon the duplicative form, on p. 111.

The prefix of this form is *te;* the verb selected, *ikiaks*, — the same verb as given by Father Cuoq to illustrate this form.

I-kiăks, I cut, in the act of cutting; *te-kiăks*, I it cut in two, or divide; *hwisk* is the Mohawk numeral *five; hwisk té-kiăks*, I cut it into five pieces: hence *te*, the prefix, cannot be a synonyme of, or a literal translation of, the Latin *bi* in *bisecto* (I cut in two), but a sign that the act of cutting is or may be repeated as often as necessary.

Again, concerning gender (p. 106): the old French missionary idea of a 'noble' and 'ignoble' gender — the former of which included 'man and deities,' and the latter 'woman, evil spirits and objects' — is explained away very satisfactorily by Mr. Hale, until he admits with them the absence of any neuter form. This leads him into the error (p. 108) of following their form of conjugation.

The model containing the verbs 'to love' and 'to see' are as given originally by Father Marcoux, and presented to the public by Father Cuoq. Here the French form of conjugation is used, which lacks the neuter pronoun 'it,' but which is supplied with the indeterminate pronoun '*on*.' The neuter pronoun, however, does exist in these dialects as presented in five different chrestomathies already prepared.

The translation of the third person neuter (p. 108), *wat-kah-tos*, by 'she sees,' should be rendered by '*it* sees;' and the third person singular, translated as indeterminate 'one sees,' is, in fact, the third person feminine; and the same mistakes occur with the verb 'to love.'

These few exceptions are simply advanced to show how much study is yet to be given to these dialects, and that we cannot accept unreservedly the opinions of even the best acknowleged authority upon languages, which, we are learning, cannot be made amenable to the grammatical rules of any known tongue.

The author's opinions concerning clans are deserving of great attention; although many will be unwilling to agree with his conclusion, that, before the division of the Iroquois into tribes, there existed but the three presented in the Book of rites. It may be true that clans in some instances have been added, but we know of many more in our own day which have died out. The last male representative of the Rhut-kun-yah clan now occupies its chieftain's seat without a single constituent, upon the Tuscarora reservation, while among the same tribe the female remnants of the snipe clan have been passed over into that of the turtle. The examples of the added Onondaga and Oneida (p. 52) among the Iroquois of eastern Canada bear directly upon some remarks from a correspondent of SCIENCE in relation to the extra clans found among those Mohawks. This subject is referred to by our correspondent as 'an interesting field of inquiry.' Mr. Hale's remarks, while suggesting a clew, are not free from objections. The clans are not called by the above names. One is termed the 'calumet,' and has the pipe as its symbol, which it was the province of one chosen from this clan to present in solemn assemblies; and the chief of this clan also named the deputies, ambassadors, etc.: hence its title of '*Ro-te-sen-na-kéh-te*,' from which name Mr. Hale evidently christens it 'Onondaga,' whose council, not tribal name, is the same, signifying 'name-bearers.' The council name of the Cayuga tribe translates literally the 'great-pipe people' (p. 79): so might there not be as feasible a foundation for naming it the Cayuga clan? Moreover, would the same reãsoning hold good concerning the rock clan, as the council

name of the Oneida tribe differs on pp. 52 and 78? Before leaving this interesting subject, we would call attention to note 5 on p. 147: "It is deserving of notice, that the titles of clanship used in the language of ceremony are not derived from the ordinary names of the animals which give the clans their designations. *Okwaho* is 'wolf;' but a man of the wolf clan is called '*Tahionni*.'" The simple explanation is, that, in both the Seneca and Oneida, '*Tai-hyo-ni*' is the name of that animal. One might be tempted to theorize upon this; but so much is yet to be learned regarding this intermingling, retention, and coining of words, that for the present we have but to collate facts which can only be clearly explained or understood by a more full and complete comparison of the Iroquois dialects than has heretofore been obtainable.

The chapter entitled the 'Book of rites' explains its origin and character, the manner of its discovery by Mr. Hale, and the character of the Indians in whose possession it was found. That it is a genuine Indian production there can be no manner of doubt; and Mr. Hale's conclusions concerning its age are in all probability correct.

The Book of rites comprises the speeches, songs, and other ceremonies, which, from the earliest period of the confederacy, are supposed to have composed the proceedings of their council when a deceased chief was lamented, and his successor installed into office. The fundamental laws of the league, a list of their ancient towns, and the names of the chiefs who constituted their first council, all chanted in a kind of litany, are also comprised in the collection. These contents are said to have been preserved in the memory for many generations, and were written down by desire of the chiefs when their language was first reduced to writing. This manuscript, the original of which had been lost, Mr. Hale has, with the most competent Mohawk assistants, translated into English, and drawn from it most interesting conclusions regarding the character and policy of the Iroquois tribes, quite dissimilar from those generally accepted. The translation, notes, and glossary exhibit the work of a careful student. In the free translation rendered by Mr. Hale to the songs, he has given them a metre almost suggesting the peculiar melody, which, in the original Mohawk, was produced by intonations; for it must be remembered, that it is one orator who must untiringly continue to sing and chant, sometimes for twenty-four hours; and only by varying his key-note is he able to accomplish this feat.

A book which is as suggestive as this must bear good fruit. We have called the attention of our readers to many disputed points in the hope of awakening a spirit of inquiry upon subjects of such vital importance, many of which are here presented for the first time. We feel assured that the hopes of the author regarding it will be fully realized, and that students of history and of the science of man will here find new material of permanent interest and value.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The evidence for evolution in the history of the extinct Mammalia.[1]

BY E. D. COPE OF PHILADELPHIA.

THE subject to which I wish to call your attention this morning requires neither preface nor apology, as it is one with the discussion of which you are perfectly familiar. My object in bringing it before the general session of the association was in view of the fact that you were all familiar with it in a general way, and that it probably interests the members of sections which do not pursue the special branch to which it refers, as well as those which do; also, since it has been brought before us in various public addresses for many years, during the meetings of this association, I thought it might be well to be introduced at this meeting of this association, in order that we might not omit to have all the sides of this interesting question presented.

The interests which are involved in it are large: they are chiefly, however, of a mental and metaphysical character; they do not refer so much to industrial and practical interests, nor do they involve questions of applied science. They involve, however, questions of opinion, questions of belief, questions which affect human happiness, I venture to say, even more than questions of applied science; certainly, which affect the happiness of the higher grades of men and women more than food or clothing, because they relate to the states of our mind, explaining as they do the reasons of our relations to our fellow-beings, and to all things by which we are surrounded, and the general system of the forces by which we are surrounded. So it has always appeared to me: hence I have selected the department of biology, and have taken a great interest in this aspect of it.

[1] A lecture given in general session, Aug. 20, 1883. Stenographically reported for SCIENCE.

The doctrine of evolution, as taught by the biologists of to-day, has several stages as grounds or parts of its presentation. First, the foundation principle is this: That the species of animals and of plants, the species of organic beings, as well as the various natural divisions into which these organic beings fall, have not always been as we see them to-day, but they have been produced by a process of change which has progressed from age to age through the influence of natural laws; that, therefore, the species which now exist are the descendants of other species which have existed heretofore, by the ordinary processes of reproduction; and that all the various structures of organic beings, which make them what they are, and which compel them to act as they now act, are the result of gradual or sudden modifications and changes during the periods of geologic time. That is the first phase or aspect which meets the naturalist or biologist.

Another phase of the question relates to the origin itself of that life which is supposed to inhabit or possess organic beings. There is an hypothesis of evolution which derives this life from no-life, which derives vitality from non-vitality. That is another branch of the subject, to which I cannot devote much attention to-day. There is still another department of the subject, which relates to the origin of mind, and which derives the mental organization of the higher animals, especially of man, from pre-existent types of mental organization. This gives us a genealogy of mind, a history of the production or creation of mind, as it is now presented in its more complex aspects as a function of the human brain. This aspect of the subject is, of course, interesting; and upon that I can touch with more confidence than upon the question of the origin of life.

Coming now to the question of the origin of structures, we have by this time accumulated a vast number of facts which have been collated by laborious and faithful workers, in many countries and during many years; so that we can speak with a good deal of confidence on this subject also. As to the phenomena which meet the student of zoölogy and botany at every turn, I would merely repeat, what every one knows, — and I beg pardon of my biological friends for telling them a few well-known truths, for there may be those present who are not in the biological section, — the phenomena which meet the student of biology come under two leading classes: the one is the remarkable fidelity of species in reproducing their like. 'Like produces like,' is the old theorem, and is true in a great many cases; just as coins are struck from the die, just as castings are turned out from a common mould. It is one of the most wonderful phenomena of nature, that such complex organisms, consisting of so many parts, should be repeated from age to age, and from generation to generation, with such surprising fidelity and precision. This fact is the first that strikes the student of these sciences. The general impression of the ordinary person would be, that these things must continue unchanged. When I began to study zoölogy and botany, I was remarkably surprised to find there was a science of which I had no conception, and that was this remarkable reproduction of types one after another in succession. After a man has had this idea thoroughly assimilated by his honest and conscientious studies, he will be again struck with another class of facts. He will find, not unfrequently, that this doctrine does not apply. He will find a series of facts which show that many individuals fail to coincide with their fellows precisely, the most remarkable variations and the most remarkable half-way attitudes and double-sided aspects occurring; and he will come to the conclusion, sooner or later, that like does not produce like with the same precision and fidelity with which he had supposed it did. So that we have these two classes of facts, — the one relating to, and expressing, the law of heredity; the other, which expresses the law of metamorphosis. I should not like to say which class of facts is the most numerously presented to the student. In the present fauna we find many groups of species and varieties before us; but how many species we have, how many genera we have, and families, we cannot definitely state. The more precise and exact a person is in his definition and in his analysis, the more definite his science becomes, and the more precise and scientific his work. It is a case of analysis and forms. What the scales are to the chemist and the physicist, the rule and measure are to the biologist. It is a question of dimension, it is a question of length and breadth and thickness, a question of curves, a question of crooked shapes or simple shapes, — rarely simple shapes, mostly crooked, generally bilateral. It requires that one should have a mechanical eye, and should have also something of an artistic eye, to appreciate these forms, to measure them, and to be able to compare and weigh them.

Now, when we come to arrange our shapes and our measurements, we find, as I said before, a certain number of identities, and a certain number of variations. This question of variation is so common and so remarkable, that it becomes perfectly evident to the specialist in each department, that like does not at all times produce like. It is perfectly clear, and I will venture the assertion that nearly all the biologists in this room will bear me witness, that variability is practically unlimited in its range, unlimited in the number of its examples, unlimited in the degree to which it extends. That is to say, the species vary by failing to retain certain characteristics, and generic and other characters are found to be absent or present in accordance with some law to be discussed farther on.

I believe that this is the simplest mode of stating and explaining the law of variation: that some forms acquire something which their parents do not possess; and that those which acquire something additional have to pass through more numerous stages than those which have not acquired so much had themselves passed through.

Of course we are met with the opposite side of the case, — this law of heredity. We are told that the facts there are not accounted for in that way; that we cannot pass from one class of facts to the other class of facts; what we find in one class is not

applicable to the other. Here is a question of rational processes, of ordinary reason. If the rules of chemistry are true in America, I imagine they are true in Australia and Africa, although I have not been there to see. If the law of gravitation is effective here, I do not need to go to Australia or New Zealand to ascertain whether it is true there. So, if we find in a group of animals a law sufficient to account for their creation, it is not necessary to know that others of their relatives have gone through a similar process. I am willing to allow the ordinary practical law of induction, the practical law of inference, to carry me over these gaps, over these interruptions. And I state the case in that way, because this is just where some people differ from me, and that is just where I say the simple question of rationality comes in. I cannot believe that nature's laws are so dissimilar, so irregular, so inexact, that those which we can see and understand in one place are not true in another; and that the question of geological likelihood is similar to the question of geographical likelihood. If a given process is true in one of the geological periods, it is true in another; if it is true in one part of the world, it is true in another; because I find interruptions in the series here, it does not follow that there need be interruptions clear through from age to age. The assumption is on the side of that man who asserts that transitions have not taken place between forms which are now distinct.

We are told that we find no sort of evidence of that transition in past geological periods; we are assured that such changes have not taken place; we are even assured that no such sign of such transition from one species to another has ever been observed, — a most astonishing assertion to make *to* a biologist, or *by* a biologist; and such persons have even the temerity to cite special cases, as between the wolf and the dog. Many of our domestic dogs are nothing but wolves, which have been modified by the hand of man to a very slight extent indeed. Many dogs, in fact, nearly all dogs, are descendants of wild species of various countries, and are but slightly modified.

To take the question of the definition of species. Supposing we have several species well defined, say four or five. In the process of investigation we obtain a larger number of individuals, many of which betray characters which invalidate the definitions. It becomes necessary to unite the four or five species into one. And so, then, because our system requires that we shall have accurate definitions (the whole basis of the system is definitions: you know the very comprehension of the subject requires definitions), we throw them all together, because we cannot define all the various special forms as we did before, until we have but one species. And the critic of the view of evolution tells us, "I told you so! There is but one species, after all. There is no such thing as a connection between species: you never will find it." Now, how many discoveries of this kind will be necessary to convince the world that there are connections between species? How long are we to go on finding connecting links, and putting them together, as we have to do for the sake of the definition, and then be told that we have, nevertheless, no intermediate forms between species? The matter is too plain for further comment. We throw them together, simply because our definitions require it. If we knew all the known individuals which have lived, we should have no species, we should have no genera. That is all there is of it. It is simply a question of a universal accretion of material, and the collection of information. I do not believe that the well-defined groups will be found to run together, as we call it, in any one geological period, certainly in no one recent period. We recognize, however, that they diverge to a wonderful extent: one group has diverged at one period, and another one has become diversified in a different period; and so each one has its history, some beginning farther back than others, some reaching far back beyond the very beginning of the time when fossils could be preserved. I call attention to this view, because it is a very easy matter for us to use words for the purpose of confusing the mind; for, next to the power of language to express clear ideas, is its power of expressing no ideas at all. As we all know, we can *say* many things which we cannot *think*. It is a very easy thing to say twice two is equal to six, but it is impossible to think it.

I would cite what I mean by variations of species in one of its phases: I would just mention a genus of snakes, Ophibolus, which is found in the United States. If we take the species of this snake-genus as found in the Northern States, we have a good many species well defined. If we go to the Gulf States, and examine our material, we see we have certain other species well defined, and they are very nicely defined and distinguished. If, now, we go to the Pacific coast, to Arizona and New Mexico, we shall find another set of species well defined indeed. If we take all these different types of our specimens of different localities together, our species, as the Germans say, all tumble together: definitions disappear, and we have to recognize, out of the preliminary list of thirteen or fourteen, only four or five. That is simply a case of the kind of fact with which every biologist is perfectly familiar.

When we come to the history of the extinct forms of life, it is perfectly true, then, that we cannot observe the process of descent in actual operation, because, forsooth, fossils are necessarily dead. We cannot perceive any activities, because fossils have ceased to act. But if this doctrine be true, we should get the series, if there be such a thing; and we do, as a matter of fact, find longer or shorter series of structures, series of organisms proceeding from one thing into another form, which are exactly as they ought to be if this process of development by descent had taken place.

I am careful to say this; because it is literally true, as we all must admit, that the system must fall into some kind of order or other. You could not collect bottles, you could not collect old shoes, but you could make some kind of a serial order of them.

There are, no doubt, characters by which such and such shoes could be distinguished from other shoes, these bottles from other bottles; but it is also true, that we have, in recent forms of life in zoölogy and botany, irrefragable proofs of the metamorphoses, and transformations, and changes of the species, in accordance with the doctrine with which we commenced.

We now come to the second chapter of our subject. With the assumption, as I take it already satisfactorily proven, of species having changed over into others, in considering this matter of geological succession or biological succession, I bring you face to face with the nature and mode of the change; and hence we may get a glance, perhaps, at its laws.

I have on the board a sketch or table which represents the changes which took place in certain of the mammalia. I give you a summary of the kind of thing which we find in one of the branches of paleontology. I have here two figures, one representing a restoration, and the other an actual picture, of two extinct species that belong to the early eocene periods. One represents the ancestor of the horse line, Hyracotherium, which has four toes on his anterior feet, and three behind; and the other, a type of animal, Phenacodus, which antedated all the horse series, the elephant series, the hog, the rhinoceros, and all of the other series of hoofed animals. Each presents us with the primitive position in which they first come to our knowledge in the history of geological time.

I have also arranged here a series of some leading forms of the three principal epochs of the mesozoic times, and six of the leading ones of the tertiary time. I have added some dat·s to show you the time when the faunae which are entombed in those beds were discovered, in the course of our studies; and you will easily see how unsafe it is to say that any given type of life has never existed, and assert that such and such a form is unknown; and it is still more unsafe, I think, to assert that any given form of life properly defined, or that a specific intermediate form of life will not be found. I think it is much safer to assert that such and such intermediate forms will be found. I have frequently had the pleasure of realizing anticipations of this kind. I have asserted that certain types would be found, and they have been found. You will see that I attend to the matter of time closely, because there have been a great many things discovered in the last ten or fifteen years in this department. In these forms I give the date of the discovery of the fauna in which they are embraced.

Here we have the White-River fauna discovered in 1856; then we skip a considerable period of time, and the next one was in 1869, when the cretaceous series was found. Six or seven cretaceous faunae have been found. Then we have the Bridger fauna in 1870, the Wasatch fauna in 1874. Next we have, in 1877, the Equus beds, and the fauna which they embrace, which also was found in 1878. The Permian fauna, which is one of the last, is 1879; and the last, the Puerco, which gives the oldest and ancestral types of the modern forms of mammalia, was only found in 1881. When I first commenced the study of this subject, about 1860, there were perhaps 250 species known. There are now something near 2,000, and we are augmenting them all the time. I have found many myself: if they were distributed through the days of the year, I think in some years I should have had several every day. But the accessions to knowledge which are constantly being made make it unsafe to indulge in any prophecies, that, because such and such things have not been found, therefore such and such things cannot be; for we find such and such things really have been and really are discovered.

The successive changes that we have in the mammalia have taken place in the feet, teeth, and brain, and the vertebral column. The parts which present us the greatest numbers of variations are those in which many parts are concerned, as in the limbs and feet. In the lower eocene (Puerco), the toes were

Formation.	No. toes.	Feet.	Astragalus.	Carpus and tarsus.	Ulnoradius.	Superior molars.	Zygapophyses.	Brain.
Miocene . . Upper . . . . (Loup Fork.)	1–1 2–2 3–3 4–4 (5–5)	Digitigrade. (Plantigrade.)	Grooved. (Flat.)	Interlocking. (Opposite.)	Faceted.	4-tubercles, crested and cemented.	Doubly involute. Singly do.	Hemispheres larger, convoluted.
Middle . . . . (John Day.)	2–2 3–3 4–4	Digitigrade.	Grooved.	Interlocking.	Faceted. Smooth.	4-tubercles, and crested.	Singly involute. Doubly do.	Hemispheres larger, convoluted.
Lower . . . . (White River.)	3–3 4–3 4–4	Digitigrade. Plantigrade.	Grooved.	Interlocking.	Smooth. Faceted.	4-tubercles, and crested.	? Singly involute.	Hemispheres small; and larger.
Eocene . . Upper . . . . (Bridger.)	3–3 4–3 4–5 5–5	(Digitigrade.) Plantigrade.	Grooved. (Flat.)	Opposite. Interlocking.	Smooth.	4-tubercles. 3-tubercles, and crested.	Singly involute. Plane.	Hemispheres small.
Middle . . . . (Wasatch.)	4–3 4–5 5–5	Plantigrade. (Digitigrade.)	Flat. (Grooved)	Opposite. Interlocking.	Smooth.	4-tubercles. 3-tubercles, a few crested.	Plane. Singly involute.	Hemispheres small; mesencephalon sometimes exposed.
Lower . . . . (Puerco.)	5–5	Plantigrade.	Flat.	Opposite.	Smooth.	3-tubercles. (4-tubercles), none crested.	Plane.	Mesencephalon exposed; hemisphere small and smoother.

5–5. In the Loup-Fork fauna, some possess toes but 1–1. Prior to this period no such reduction was known, though in the Loup-Fork fauna a very few species were 5–5. Through this entire series we have transitions steady and constant, from 5–5, to 4–5, to 4–4, to 4–3, to 3–3, to 2–2, to 1–1. In the Puerco period there was not a single mammal of any kind which had a good ankle-joint; which had an ankle-joint constructed as ankle-joints ought to be, with tongue and groove. The model ankle-joint is a tongue-and-groove arrangement. In this period they were all perfectly flat. As time passes on we get them more and more grooved, until in the Loup-Fork fauna and the White-River fauna they are all grooved. In the sole of the foot, in the Puerco fauna, they are all flat; but in the Loup-Fork fauna the sole of the foot is in the air, and the toes only are applied to the ground, with the exception of the line of monkeys, in which the feet have not become erect on the toes, and the elephant, in which the feet are nearly flat also, and the line of bears, where they are also flat. As regards the ungulation between the small bones of the palm and of the sole, there is not a single instance in which the bones of the toes are locked in the lower eocene, as they are in the later and latest tertiary.

When we come to the limbs, the species of the Puerco fauna have short legs. They have gradually lengthened out, and in the late periods they are nearly all relatively long.

Coming to the vertebrae as a part of the osseous system, I will mention the zygapophyses, or antero-posterior direct processes, of which the posterior looks down, and the anterior looks up. They move on each other, and the vertebral column bends from side to side. In the lower forms of mammals they are always flat, and in the hoofed mammals of the Puerco period they are all flat. In the Wasatch period we get a single group in which the articulation, instead of being perfectly flat, comes to be rounded; in the later periods we get them very much rounded; and finally, in the latest forms, we get the double curve and the locking process in the vertebral column, which, as in the limb, secures the greatest strength with the greatest mobility. In the first stages of the growth of the spinal cord, it is a notochord, or a cylinder of cartilage or softer material. In later stages the bony deposit is made in its sheath until it is perfectly segmented.

Now, all the Permian land-animals, reptiles, and batrachians retain this notochord with the beginnings of osseous vertebrae, in a greater or less degree of complexity. There are some in South Africa, I believe, in which the ossification has come clear through the notochord; but they are few. This characteristic of the Permian appears almost alone, — perhaps absolutely alone as regards land-animals. There is something to be said as to the condition of that column from a mechanical standpoint, and it is this: that the cord exists, its osseous elements disposed about it; and in the batrachians related to the salamanders, and the frogs, these osseous elements are arranged under the sheath in the skin of the cord; and they are in the form of regular concave segments, very much like such segments as you will take from the skin of an orange, — parts of spheres, and having greater or less dimensions according to the group or species. Now, the point of divergence of these segments is on the side of the column. They are placed on the side of the column where the segments separate, — the upper segments rising and the lower segments coming downward. To the upper segments are attached the arches and their articulations, and the lower segments are like the segments of a sphere. If you take a flexible cylinder, and cover it with a more or less inflexible skin or sheath, and bend that cylinder sidewise, you of course will find that the fractures of that part of the surface will take place along the line of the shortest curve, which is on the side; and, as a matter of fact, you have breaks of very much the character of the segments of the Permian batrachia. It may not be so symmetrical as in the actual animal, for organic growth is symmetrical so far as not interfered with; for, when we have two forces, the one of growth and the other of change or alteration, and they contend, you will find in the organic being a quite symmetrical result. That is the universal rule. In the cylinder bending in this way, of course the shortest line of curve is right at the centre of the side of that cylinder, and the longest curve is of course at the summit and base, and the shortest curve will be the point of fracture. And that is exactly what I presume has happened in the case of the construction of the segments of the sheath of the vertebral column in the lateral motion of the animal swimming, always on one side, and which, at least, has been the actual cause of the disposition of the osseous material in its form. I have gone beyond the state of the discussion in calling attention to one of the forces which have probably produced this kind of result. That is the state of the vertebral column of many of the vertebrata of the Permian period.

I go back to the mammalia, and call attention to the teeth. The ordinary tooth of the higher type of the mammalia, whether hoofed or not, with some exceptions, is complex with crests or cusps. In cutting the complex grinding surfaces we find they have been derived by the infolding extensions of four original cusps or tubercles. They have been flattened, have been rendered oblique, have run together, have folded up, have become spiked, have descended deeply or have lifted themselves, so that we have teeth of all sorts and kinds, oftentimes very elegant, and sometimes very effective in mechanism. In many primary ungulates, the primitive condition of four conical tubercles is found. In passing to older periods we find the mammalia of the Puerco period, which never have more than three tubercles, with the exception of three or four species. In the succeeding periods, however, they get the fourth tubercle on the posterior side. Finally, you get a complicated series of grinding or cutting apparatus, as the case may be.

Last, but not least, we take the series of the brain. No doubt the generalization is true, that the primitive forms of mammalia had small brains with smooth

hemispheres; later ones had larger brains with complex hemispheres. In general, the carnivora have retained a more simple form of brain, while herbivorous animals have retained a most complicated type of brain. The lowest forms of mammalia display the additional peculiarity of having the middle brain exposed; and the hemispheres or large lobes of the brain, which are supposed to be the seat of the mental phenomena, are so reduced in size at the back end that you see the middle brain distinctly, though it is smaller than in reptiles and fishes. It is beyond the possibility of controversy, that these series have existed, and that they have originated in simplicity, and have resulted in complication; and the further deduction must be drawn, that the process of succession has always been towards greater effectiveness of mechanical work. There are cases of degradation, as in the growing deficiency in dentition in man. There is no doubt that a large number of people are now losing their wisdom-teeth in both jaws.

We are now brought to the question of the relations which mind bears to these principles. The question as to the nature of mind is not so complex as it might seem. There is a great deal of it, to be sure; but on examination it resolves itself into a few ultimate forms. An analysis reduces it to a few principal types or departments, — the departments of the intelligence and of emotions (with their modified smaller forms, likes and dislikes), and the will, if such there be. Those three groups, proposed by Kant, are well known, and adopted by many metaphysicians; and they stand the scrutiny of modern science perfectly well in both men and the lower animals. But the question of the material of the mind, the original raw stuff out of which mind was made, is one which is claiming attention now from biologists, as it always has done from physiologists proper and physicians. This is sensibility, mere simple sensibility, unmodified sensibility or consciousness. Sensibility, in connection with memory, is sufficient for the accomplishment of wonderful results. It is only necessary to impress the sensibility with the stimuli which this world affords, whether from the outside or the inside, to have the record made, and to have the record kept. Among wonderful things this is perhaps the most wonderful: that any given form of matter should be able to retain a record of events, a record which is made during a state of sensibility for the most part, a greater or less degree of sensibility, which is retained in a state of insensibility, and is finally returned to the sensibility by some curious process of adhesion, and the results of impresses which are found on the material tissue concerned.

And these simple elements of mind are found in animals. No zoölogist who has perception or honesty, nor any farmer or breeder, nor any person who has charge of animals in any way, can deny sensibility to all the lower animals at times. The great stumbling-block in the way of the thinker in all this field is the great evanescence of this sensibility: the great ease with which we dissipate it, the readiness with which we can deprive a fellow-being of his sense, is a stumbling-block in more ways than one. While it is a question of the greatest difficulty, nevertheless, like other departments of nature, doubtless it will ultimately be explained by the researches of physiologists. I only need to call attention to the fact as an important factor in evolution.

Of course, if these structures are suggested, affecting the mechanical apparatus, the question arises, whether they were made ready to hand, whether the animal, as soon as he got it, undertook to use it, and whether he undertook to use the organism under the dire stimuli of necessity, or amended through ages these modifications in his own structure. We are told by some of our friends, that law implies a lawgiver, that evolution implies an evolver: the only question is, Where is the lawgiver? where is the evolver? where are they located? I may say, it is distinctly proven in some directions, that the constant applications of force or motion in the form of strains, in the form of impacts and blows, upon any given part of the animal organism, do not fail to produce results in change of structure. I believe the changes in the ungulates to which I have called your attention are the result of strains and impacts, precisely as I have shown you the manner of the fracture of the vertebral column of the primitive vertebrates of the Permian period. This would require long discussion to render clear: nevertheless, I venture to make the assertion that this series of structures is the result of definite and distinct organic forces, directed to special ends. We have yet to get at the conflicting forces which have produced the results we see. Mechanical evolution will give us a good deal to do for some time to come. Of course, if motion has had an effect in modifying structure, it behooves us to investigate those forces which give origin to motion in animals. First in order come the sensibilities of the animal, which we have traced to simple consciousness; stimuli, upon notice of which he immediately begins to move. The primary stimulus of all kinds of motion is necessarily touch. If a stone falls upon the tail of some animal which has a tail, he immediately gets out of that vicinity. If a jelly-fish with a stinging apparatus runs across an eel which has no scales, the eel promptly removes. External applications of unpleasant bodies will always cause an animal to change his location. Then he is constantly assaulted by the dire enemy of beasts, hunger, an instinct which is evidently universal, to judge from the actions of animals. This seems to have fashioned, in large part, all forms of life, from the least to the greatest, from the most unorganized to the most complex. Each exercised itself for the purpose of filling its stomach with protoplasm. Then come the stimuli, which should be included under the class of touch, changes of temperature. No animals like to be cold or too hot; and when the temperature is disagreeable, the tendency is to go away from that locality. Among primary instincts must be included that of reproduction. After that comes the sensation of resistance, or, carried to a high degree, of anger: when an animal's interests are interfered with, its movements re-

stricted, it prompts to the most energetic displays. So, you see, it is a matter of necessity that mental phenomena lie at the back of evolution, provided always that the connecting link of the argument — that motion has ever affected structure — be true. That is a point which, of course, admits of much discussion. I have placed myself on the affirmative side of that question; and, if I live long enough, I expect to see it absolutely demonstrated.

Of course the development of mind becomes possible under such circumstances. It is not like a man lifting himself up by his boots; which it would be if he had no such thing as memory. But with that memory which accumulates, which formulates first habits, and then structures, especially in the soft, delicate nervous tissue, the development of the mind as well as the machinery of the mind becomes perfectly possible. We develop our intellect through the accumulation of exact facts; through the collation of pure truth, no matter whether it be a humble kind of truth, — as the knowledge of the changes of the seasons, which induces some animals to lay up the winter's store, — whether it be knowledge of the fact that the sting of the bee is very unpleasant, or knowledge of the fact (of which the ox, no doubt, is thoroughly aware) that the teeth of the wolf are not pleasant to come in contact with; or whether it be the complex knowledge of man. When the cerebral matter has become larger and more complex, it receives and retains a much greater number of impressions, and the animal becomes a more highly educated being.

As regards the department of emotions or passions, it is also much stimulated by the environment. Animals which live in a state of constant strife, naturally have their antagonistic passions much developed; while amiable, sympathetic sentiments are better and more largely produced by peace-loving animals. Thus it is that the various departments of the mind have the beautiful results which we now find in the human species.

There are some departments of the mind which some of our friends decline to admit having had such an origin. The moral faculty, for instance, is excepted by many from this series. But the reasons why they object to its production in this way are, to my mind, not valid. The development of the moral faculty, which is essentially the sense of justice, appears to them not to fall within the scope of a theory of descent or of evolution. It consists of two parts. First is the sentiment of benevolence, or of sympathy with mankind, which gives us the desire to treat them as they should be treated. It is not sufficient for justice that it is unmixed mercy, or benevolence, which is sometimes very injurious, and very often misplaced. It requires, in the second place, the criticism of the judgment, of the mature intellect, of the rational faculty, to enable the possessor to dispose of his sentiments in the proper manner. The combination of rational discrimination and true judgment, with benevolence, constitutes the sense of justice, which has been derived, no doubt, as a summary of the development of those two departments of the mind, — the emotions and the intellect.

It is said, that a sense of justice could not be derived from the sense of no justice; that it could not have been derived from the state of things which we find in the animals, because no animal is known to exhibit real justice: and that objection is valid as far as it goes. I suspect that no animal has been observed to show a true sense of justice. That they show sympathy and kindness, there is no question; but when it comes to real justice, they do not display it. But do all men display justice? Do all men *understand* justice? I am very sure not. There are a good many men in civilized communities, and there are many tribes, who do not know what justice is. It does not exist as a part of every mental constitution. I never lived among the Bushmen, and do not know exactly what their mental constitution is; but in a general way the justice of savages is restricted to the very smallest possible circle, — that of their tribe or of their own family. There is a class of people who do not understand justice. I do not refer to people who know what right is, and do not do it; but to the primitive state of moral character, in which, as in children, a sense of justice is unknown. I call attention to the fact, because some of our friends have been very much afraid that the demonstration of the law of evolution, physical and metaphysical, would result in danger to society. I suspect not. The mode in which I understand this question appears to me to be beneficial to society, rather than injurious; and I therefore take the liberty of appending this part of the subject to its more material aspect.

To refer to another topic, and that is to the origin of life, the physical basis of life. The word 'life' is so complex that it is necessary to define it, and so to define it away that really the word 'life' does not retain its usual definition. Many phenomena of life are chemical, physical, mechanical. We have to remove all these from consideration, because they come within the ordinary laws of mechanical forces; but we have a few things left which are of a different character. One is the law of growth, which is displayed in the processes of embryonic succession; secondly, the wonderful phenomena of sensibility. Those two things we have not yet reduced to any identity with the ordinary laws of force. In the phenomena of embryology the phenomena of evolution are repeated, only concentrated in the early stages through which animals have to pass. So whatever explains the general phenomena of evolution explains the phenomena of embryology.

What is the nature of physical sensibility? In this planet, it is found residing only in one form of matter, which has a slightly varied chemical constitution, namely, protoplasm ; so-called from a physical standpoint. Now, this world, as you all know, has passed through many changes of temperature. Its early periods, it is probable, were so very hot that protoplasm had a very poor chance. The earth has passed through a great many changes of temperature, many of which would not permit the existence of protoplasm. Again, can we assume for a moment that this little speck in the great universe is the only seat of life? I suppose scarcely any scientific man

will venture to do so. If, therefore, life exists in other parts of this great universe, does it necessarily occupy bodies of protoplasm in those different, remote spheres? It would be a great assumption. It is altogether improbable. The certainty is, that in those planets which are in proximity to the sun's heat there could be no protoplasm. Protoplasm in the remote planets would be a hard mineral, and near the sun it would be dissipated into its component gases. So that, if life be found in other parts of this universe, it must reside in some different kind of material. It is extremely probable that the physical conditions that reside in protoplasm might be found in other kinds of matter. It is in its chemical inertness, and in its physical constitution, that its adaptation to life resides; and the physical constitution necessary for the sustentation of life may be well supposed to exist in matter in other parts of the universe. I only say the door is open, and not closed: any one who asserts that life cannot exist in any other material basis than protoplasm is assuming more than the world of science will permit him to assume. And that it is confined to this single planet, and not in the great systems of the universe, — that assumption will not for a moment be allowed. Therefore the subject is one which allows us a free field for future investigation: it is by no means closed in the most important laws which it presents to the rational thinker. I hope, therefore, that, if the evidence in favor of this hypothesis of the creation of living forms be regarded as true, that no one will find in it any ground for any very serious modification of existing ideas on the great questions of right and wrong which have long since been known by men as a result of ordinary experience, and without any scientific demonstration whatsoever.

A classification of the natural sciences.[1]

BY T. STERRY HUNT, LL.D., F.R.S., OF MONTREAL.

To frame a rational classification of the natural sciences, and to define their mutual relations, have often been attempted. The present writer, in an essay read before the National academy of sciences in April, 1881, and since published in the Philosophical magazine, with the title of 'The domain of physiology,' suggested the basis of such a scheme, and now, at the request of some of his readers, ventures, for the first time, to embody in a concise and tabulated form the views then and there enunciated, in the hope that other students may find it not unworthy of their notice.

The study of material nature constitutes what the older scholars correctly and comprehensively termed physics (the words 'physical' and 'natural' being synonymous), and presents itself in a twofold aspect, — first, as descriptive; and, second, as philosophical, — a distinction embodied in the terms 'natural history' and 'natural philosophy,' or, more concisely, in the words 'physiography' and 'physiology.' The latter word has been employed, in this general sense, to designate the philosophical study of nature from the time of Aristotle, and will so be used in the present classification.

The world of nature is divided into the inorganic or mineralogical, and the organic or biological, kingdoms; the division of the latter into vegetable and animal being a subordinate one. The natural history, or physiography, of the inorganic kingdom, takes cognizance of the sensible characters of chemical species, and gives us descriptive and systematic mineralogy, which have hitherto been restricted to native species, but, in their wider sense, include all artificial species as well. The study of native mineral species, their aggregations, and their arrangement as constituents of our planet, is the object of geognosy and physical geography. The physiography of other worlds gives rise to descriptive astronomy.

The natural philosophy of the inorganic kingdom, or mineral physiology, is concerned, in the first place, with what is generally called dynamics or physics; including the phenomena of ordinary motion, sound, temperature, radiant energy, electricity, and magnetism. Dynamics, in the abstract, regards matter in general, without relation to species; chemism generates therefrom mineralogical or so-called chemical species, which, theoretically, may be supposed to be formed from a single elemental substance, or *materia prima*, by the chemical process. Dynamics and chemistry build up our inorganic world, giving rise to geogeny, and, as applied to other worlds, to theoretical astronomy.

Proceeding next to the organic kingdom, its physiographical study leads us first to organography, and then to descriptive and systematic botany and zoölogy, two great subdivisions of natural history. Coming, then, to consider the physiological aspect of organic nature, we find, besides the dynamical and chemical activities manifested in the mineral, other and higher ones which characterize the organic kingdom. On this higher plane of existence, are found portions of matter which have become individualized, exhibit irritability, the power of growth by assimilation, and of reproduction, and which establish relations with the external world by the development of organs, all of which characters are foreign to the mineral kingdom. These new activities are often designated as vital; but since this word is generally made to include at the same time other manifestations which are simply dynamical or chemical, I have elsewhere proposed for the activities characteristic of the organism the term biotics (βιοτικός, pertaining to life). The physiology of matter in the abstract is dynamical, that of mineral species is both dynamical and chemical, while that of organized forms is at once dynamical, chemical, and biotical. All of these, I may remark, I regard as successive manifestations of an energy inherent in matter.

The study of the biotical activities of matter leads to organogeny and morphology, while the relations of organisms to one another and to the inorganic kingdom give us physiological botany and zoölogy. We thus arrive at a comprehensive and simple scheme of the natural sciences, which I have endeavored to set forth in the subjoined table.

[1] Abstract of paper read in general session, Aug. 17, 1883.

Natural Sciences;	Inorganic Nature;	Organic Nature.
Descriptive. General Physiography, or Natural History.	Mineral Physiography. Descriptive and Systematic Mineralogy; Geognosy; Geography; Descriptive Astronomy.	Biophysiography. Organography; Descriptive and Systematic Botany and Zoölogy.
Philosophical. General Physiology, or Natural Philosophy.	Mineral Physiology. *Dynamics* or *Physics; Chemistry.* Geogeny; Theoretical Astronomy.	Biophysiology. *Biotics.* Organogeny; Morphology; Physiological Botany and Zoölogy.

PROCEEDINGS OF SECTION A. — MATHEMATICS AND ASTRONOMY.

PAPERS READ BEFORE SECTION A.

[*Continued.*]

Orbit of the great comet of 1882.

BY EDGAR FRISBIE OF WASHINGTON, D.C.

This is a partial record of observations at Washington. Mr. Winlock is preparing a description of all the physical phenomena of the comet which were there observed. The first Washington observation of the comet was at two o'clock on a September afternoon, and a comparison was then made with the position of the sun. Good observations were obtained on the meridian for three days. The calculations from these served to fix the place of the comet with fair approximate accuracy for three months, which was a somewhat remarkable success. Afterward a difficulty occurred in obtaining accurate observations; because there were several different points of light presented in an ill-defined nucleus, and it was uncertain whether the observations always referred to the same luminous point. These observations were made in October and November. The following ephemeris was calculated: —

Sept.	17.2282			ϕ	89° 13′ 42.70″
Ω	346°	1′	7.91″	log. a	1.9331366
$\pi\ \Omega$	69	36	12.79	log. q	7.8904739
i	141	59	52.16	period	793.689

The author compared the foregoing with the observations of other astronomers. The most prominent variation was in respect to the period, which others gave as 659, 997, 852, and 654 years. A contrivance was exhibited, showing the respective positions of the earth and comet, and their directions of motion, by means of pasteboard planes attached at an angle.

The rotation of domes.

BY G. W. HOUGH OF CHICAGO, ILL.

Observatory domes are in general very heavy. As they grow old, owing to the settling of walls and other changes, they are apt to become almost unmanageable. The dome at Chicago is very weighty, every thing about the observatory being built in a very substantial manner. When Dr. Hough first tried to move the dome, he found its two sides working with unequal friction; and this was afterward remedied to some extent, but by no means fully. About two months ago a gas-engine was placed in position to revolve the dome. It was a great satisfaction to see the dome go round continuously, without hitches. The cost of moving the dome by such means is a mere trifle, aside from the first cost of the engine. The use of water-power where that was easily accessible must, however, be preferred in many instances where a sufficient head is supplied by street mains.

Dr. C. A. Young said, in discussing the foregoing, that when he came to Princeton he found a very heavy dome there. One man, using thirty pounds pressure on a two-foot crank, was very tired after giving the dome one turn. A gas-engine has since been put in below, and the power is communicated by a belt. A revolution can be made in four minutes, and the shutter raised in two. In general, the dome is placed and the shutter opened within five minutes. Dr. Young expressed a hope that the Brush storage batteries would furnish electrical illumination and power for the work of observatories, as the electricity might be stored even from a gas-engine operating a dynamo during hours of the day when there was no other use for its power. At present the direct action of a gas-engine on a dynamo, with no intervention between the dynamo and the light, was too irregular to serve the purpose.

Descriptive-geometrical treatment of surfaces of the second degree.

BY J. BURKITT WEBB OF ITHACA, N.Y.

For the purpose of greater conciseness the speaker confined his remarks to the general ellipsoid, remarking that the usual treatment of problems upon this surface — as, for instance, such problems as finding the shade and shadow, or drawing tangent planes — is lacking in generality; the body being taken in such

special position, or referred to such special axes, as reduce the general problem to a specially simple one.

The speaker then drew the projections of three conjugate diameters of a general ellipsoid upon the board, stating that this was the best method of defining that body. He then proceeded to find the projections of the enveloping cylinders, and the shadow of the body; which he showed could as easily be done for the general ellipsoid, in a perfectly general position, as for special cases. In fact, it appeared that problems on this body gained nothing in simplicity by special methods and devices which detract from the generality of the treatment.

List of other papers.

The following additional papers were read in this section, some of them by title only: Tidal observations on soundings distant from shore, by *J. M. Batchelder*. Investigation of light variations of Sawyer's variable, by *S. C. Chandler*. Standard time-pointer and a time longitude dial; System of algebraic geometry, by *Samuel Emerson*. The calculus of direction and position, by *E. W. Hyde*. Observations on the transit of Venus made at Columbia college; Description of the new observatory at Columbia college, by *J. K. Rees*. The light variations of T. Monocerotis, by *E. F. Sawyer*. Method of observing eclipses of Jupiter's satellites, by *D. P. Todd*. Conic sections in descriptive geometry, by *J. B. Webb*. Descriptive geometry applied to the general ellipsoid, by *C. M. Woodward*. Some observations on Uranus, by *C. A. Young*.

PROCEEDINGS OF SECTION B. — PHYSICS.

PAPERS READ BEFORE SECTION B.

[*Continued.*]

The tornado at Racine, May 18, 1883.

BY P. R. HOY OF RACINE, WIS.

A curious mistake preceded the reading of this paper. There was some confusion between the abstracts of this and another paper on a tornado, which were submitted to the sectional committee; and the other paper was entered on the daily programme, but was withdrawn.

Mr. Hoy's paper began by stating that the early part of the day was pleasant, but about 6.45 in the evening two clouds of ominous appearance joined, from opposite quarters of the heavens, and at once the cyclone began. Its general direction was to the north of east. There was no rain at Racine with the storm, but there was noticed a very strong odor of ozone while the cyclone was at its height. At the start it was barely two rods wide, but when it reached Racine it had expanded to twenty rods. Its motion was rotary and oscillatory, and all *débris* was thrown to the centre of the track. When the cyclone crossed the lake it formed huge waterspouts, one central, and seven to eight accessory, whirling about the main trunk.

Prof. H. A. Rowland proceeded to discuss the paper as follows: Most observers of tornadoes just perceive that there is a whirling motion of the air, and it knocks down objects, and that is the principal thing they see. But that is very ordinary observation. Of course, a column of air in such swift rotation will tear houses down, spurt water up, and do every thing of that sort. The particular point which I observed in this paper was the description of the formation of the tornado. The phenomenon which is to be explained is the formation of the tornado, and very few have observed this. This description was very short; merely, that, over in the west or south-west, the clouds formed. Of course, to an observer from the west, one would appear north, and the other south. The point I wish to bring out is, that there was lightning passing between the two clouds. In Mr. Finley's description of six hundred tornadoes, I do not see any similar account. Many observers have seen lightning play around these clouds, but not passing between the two clouds. Mr. Finley applied to me to know whether there was any thing in the electrical theory of a tornado. Of course, any theory of the destruction being caused by electricity, houses being attracted, etc., — all that is mere nonsense. We know that the attraction of electricity is only a mere fraction of an ounce to the square inch. Before the force becomes sufficient to raise a great weight, a spark passes, and a discharge of electricity takes place. But in this case (these two clouds passing from north to south, and boiling up, having flashes of lightning playing round them), I thought there might be something in the electrical theory, as far as formation was concerned; and I calculated for the signal-service and Mr. Finley what amount of energy there was in two clouds approaching each other in this way. The rotation of the earth will cause them to come together, not in a straight line, but a little aside from each other, forming a spiral motion. The direction of the rotation of the tornado is a necessary consequence of the earth's rotation: so that it might be possible to have these electrified clouds approach each other by mutual attraction, and form a tornado at the point where they meet. I calculated the energy, and found there was sufficient for a rather small tornado in the case I took. I would not be willing to say that is the theory of all tornadoes. I say that it is only possible. There is a great deal more energy in a mass of air heated up to a considerable temperature, and rising, by force of gravitation, — a great many times more. If it were not for the electrical phenomena observed in the case, I should say there was very little probability of the electrical theory. I believe Mr. Finley will direct the signal-service observers to watch the direction of the wind. If it flows in from all directions at the point where the tornado is formed, we should determine it to be due to the rise of hot air at that point. When the ground is very hot and the

air very sultry, we have two causes; and it is only by observation that we can find out its true manner. I do not lay very much stress upon the electrical theory. But it is an interesting point, to me, to notice that flashes of lightning have been observed between these two clouds, showing that they were differently electrified, and that there was some plausibility for the theory which I sent to the signal-service.

Prof. F. E. Nipher continued this discussion the next day, as follows: One matter connected with the effects of this tornado contained a point, it seems to me, of sufficient interest to call the attention of observers to the matter, in case any one should have an opportunity to observe the effect of a tornado upon water. Mr. Ferrel, I think, in his description of a tornado, states that we have a rising of the water, forming a sort of cone in the centre of the tornado; the effect being, of course, ascribed to the diminution of pressure which is known to be there. In the cyclone proper, where we have a large area, we have a storm-wave as the principal element in the case, and there is an upheaval of the water in the area of low pressure. In the tornado it seems to me very questionable whether that occurs. I base that upon this observation: A smaller wind-whirl which was observed by myself in northern Missouri, which was rather violent though not destructive, — a column of dust several hundred feet high being raised, — passed out upon a pond of water five or six feet deep, and a depression was formed in the water, extending to the bottom of the pond, — an immense cup. The water was revolving rapidly; and it was thrown into rotation with a centrifugal effect, — the same effect as when a vessel is whirled. It seems to me that this is an element which has not been considered as it should be. If the whirl is small, and you have not only a diminution of pressure in the centre, but of the whole body of the water, the friction producing a rotation of the water, if the result is sufficiently small you might get a depression instead of an elevation. I call attention to this, so that those who may be fortunate enough to see a tornado on the water may not take it for granted that it is all known.

As to the remarks of Professor Rowland in regard to the possible electrical origin of a tornado, I know that he was very careful to say that he did not think any of the destructive effects could be ascribed to the action of electricity. I gathered the idea that he thought a tornado might originate in that way, — that two electrified clouds will attract each other, and come together; and he calculates the energy of the attraction which bodies can have for each other in air. It seems to me that the simple observation that was made by Mr. Hoy, together with another fact which we know, — that when the discharge passes between electrified bodies they are almost wholly discharged, — would show that when that happens the cause for that motion has disappeared. When these two clouds approach, a spark passes, and the whole thing is gone. So long as there is no spark passing, we know very well that the attraction is very much less than the maximum attraction of $\frac{16}{100}$ of an ounce on the square inch. I think, perhaps, that is a matter Professor Rowland did not consider. It does not seem to me at all likely that any such origin can be ascribed to the tornado. When it is developed, you may have a rarefied column which may be very highly rarefied, connecting the earth with the upper regions, which is precisely the reason that the lightning which was observed in the case of the Racine tornado was not accompanied by thunder.

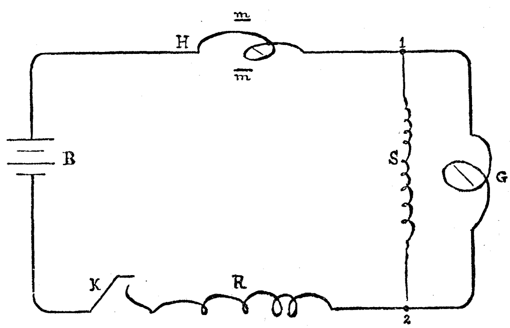
Prof. J. T. Lovewell said it occurred to him, from his observation, that a good deal of care is necessary in order that the observer may know exactly what he sees. It was my fortune, said he, to witness a small whirl at a distance of three or four miles. I saw the funnel-shaped cloud descend toward the earth, and it looked to me as though there were a column of water. Many people who saw it spoke of it as a waterspout. It might have been water, for aught that we could have said from our point of sight. I immediately drove to the spot, and it appeared that not a drop of rain had fallen in that track. The whirl had been sufficient to overturn a few stacks of grain and hay, and a man was thrown about with his team in the road. I think, if it had struck a body of water, I should be slow to believe that it lifted any solid column of water into the air one hundred feet. It would have made a grand scattering of the water, and a great deal of it would have been thrown up into the air. I believe that a good deal of that which is commonly ascribed to columns of water rising up, and pouring down the sides in cataracts, is optical illusion. I should be slow to take the testimony of a person seeing them, unless he had his mind disabused of the common notions about these waterspouts. So far as their electrical origin is concerned, I quite agree with Professor Nipher that it is not by any means proven that electricity has any thing to do with them, except that it is a necessary adjunct, of course, to all such disturbances.

A method for the calibration of a galvanometer.

BY B. F. THOMAS OF COLUMBIA, MO.

A BATTERY of any sort is joined in circuit with a sensitive galvanoscope H, a galvanometer G, and any variable resistance R. When the circuit is closed at K, the current is so adjusted by varying R, as to give the highest desirable deflection of the galvanometer needle. The needle of H will be forced against the stops. By means of magnets m and m, the needle of H is brought back to zero. If these magnets and the galvanoscope be undisturbed, the original current strength will be indicated when the needle stands at zero, whatever changes may have been made in the circuit. If now the shunt S be connected at 1, 2, and the resistance of the shunt is made equal to that of the galvanometer (positively determined), and the needle of H brought back to zero (by increasing R, as insertion of the shunts lowers the total resistance of the circuit, and therefore increases the current strength, deflecting H), a new deflection of the galvanometer needle will be produced, the deflection

being that due to a current of one-half the strength of the original current. By giving to S values equal to ∞, 3, 2, 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc., times the resistance of G, and bringing the needle of H to zero each time, deflection of G will result, due to currents whose strengths are as 1, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, etc. The curve is then plotted with deflections and current strengths as co-ordinates. Any desired number of points in the



curve may be obtained by giving S the proper values. The calibration may be checked by making a new adjustment for the united current, so that the deflection of G shall be about two-thirds the first deflection, and proceeding as above. Plotting the new values obtained, the curves will coincide if the work is correct. If it be found desirable, the battery may be exchanged for another during the determination.

The utilization of the sun's rays for warming and ventilating apartments.

BY E. S. MORSE OF SALEM, MASS.

MR. MORSE drew attention to this device a year ago, before the National academy of sciences. At that time he was able to offer only crude computations as to the operations of the heater, derived from its use at the museum of Salem, Mass.

The device consists mainly of a slaty surface painted black, standing vertically upon a wall, outside the building, with flues to conduct warmed air to the inside. The slates are inserted in a groove, much as one might place glass in a frame. One made within the last year was three feet wide and eight long. It was placed where it received the sun's rays as directly as practicable. Its service was to warm a room used for a library. During an entire winter the room was thus made comfortable, except on a few of the coldest days. The current of air passing through it, when the sun's rays impinged directly upon it, was raised about 30°; it discharged 3,206 feet of warmed air in an hour. This was in the morning. At 11.45 the air of the apartment was raised 29°, with 3,326 cubic feet of air discharged; at 12.45, 29° and 4,119 feet; at 1.55, 24° and 3,062 feet; at 2.45, 20° and 1,299 feet. The room measured 20×14, and was ten feet high.

The apparatus works to most advantage in a room that is ventilated by an open chimney. But some very good results have been obtained in closed rooms. One was cited, where the air in a public building was raised by such means to nearly 40° above the outside temperature. In general, a difference of 30° to 35° can thus be secured during four or five working hours of the day.

Professor Mendenhall stated that he had seen the working of the apparatus, and it proved very satisfactory. Professor Rogers gave similar testimony.

New form of selenium cell, with some remarkable electrical discoveries made by its use.

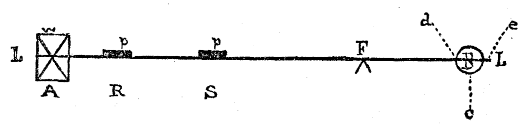
BY C. E. FRITTS, OF NEW YORK.

PROFESSOR MENDENHALL stated that in the absence of the author he was able to give only a brief summary of the paper. In the ordinary method of making selenium cells, they are constructed of a great many portions put side by side; the resistances are necessarily very high in these cells, and the light is allowed to strike in the direction of a right angle to the direction of the passage of the current. Mr. Fritts seems to have devised a different mode of operating these cells by using a very large surface, and in that way has succeeded in diminishing the resistance very greatly, which is very desirable. He has resistance as low as nine or ten ohms in the dark. The radical point of difference is, that in this case the light is allowed to strike upon the cell in the same direction as the current. He states that he has discovered many remarkable properties by means of his investigations with the instrument. When a cell of this kind breaks down, it can easily be remedied and repaired: in fact, there is no danger or difficulty of their breaking down permanently.

A method of determining the centre of gravity of a mass.

BY B. F. THOMAS OF COLUMBIA, MO.

A BAR, LL, is balanced on a knife-edge, F, so as to form a very sensitive balance. The body, B, of mass, M, is placed with a marked point in contact with a fine point, d; and another body of mass, W, is placed



at A, so as to nearly balance B. A small body of mass, p, is placed at S, to complete equilibrium. B is then rotated 180° horizontally, bringing its marked spot in contact with a second fixed point, E. Equilibrium is restored by placing p at R. The equations of moments in the two positions are, respectively,—

$$W \times AF + p \times SF = M\,(Fd + dc);$$

(c being centre of gravity); and

$$W \times AF + p \times RF = M\,(Fe - dc); \; (ec = dc).$$

Subtracting the first equation from the second,—

$$p \times RS = M\,(de - 2\,cd);$$

$$\therefore\; cd = \frac{de}{2} - \frac{p \times RS}{2\,M};$$

cd is therefore the distance from the marked spot

to a vertical plane containing the centre of gravity. Taking a second marked spot in the plane thus found, the operation is repeated, with the plane horizontal. This gives a second plane through the centre of gravity. A third operation, with the intersection of the two planes in the line *de*, locates the centre of gravity.

The kinetic theory of the specific heat of solids.

BY H. T. EDDY OF CINCINNATI, OHIO.

THIS paper was based upon the well-known views of its author respecting the use to be made of the different degrees of freedom of motion among the atoms of solid bodies, in deducing a theory that will explain their diverse powers of conducting heat, and of transmitting or causing the transmission of radiant energy. The theory is based upon the conception that all bodies are constituted of equal ultimate atoms, whose combination, in different degrees of freedom, in different molecules, gives rise to the characteristic differences of elementary substances. This paper shows that the same hypothesis would cause solids, which are kept in equilibrium by radiation, to be also in thermal equilibrium when brought into contact; the equilibrium depending upon collisions of the molecules.

A kinetic theory of melting and boiling.

BY H. T. EDDY OF CINCINNATI, OHIO.

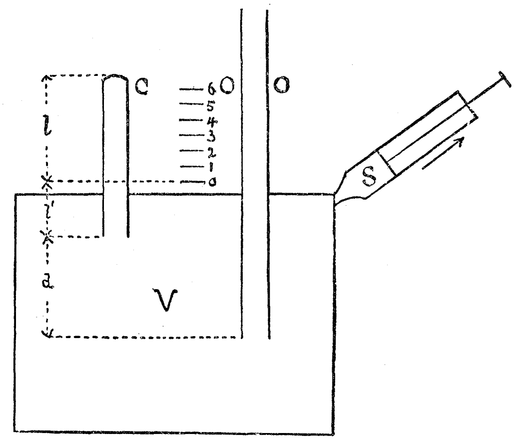
IN a solid in which the molecules are evidently held at nearly fixed mean distances by cohesive and elastic forces, there are two kinds of partially constrained freedom of motion possible for each molecule as a whole: first, a motion of its centre in a small orbit of more or less irregular shape about a mean position; and, second, a more or less irregular pendular motion of oscillation about a mean directional position. Both of these motions can be treated as vibratory motions; and the laws of force under which the motions occur, though somewhat unlike, have a general resemblance.

Two forms of apparatus for Boyle's law.

BY B. F. THOMAS OF COLUMBIA, MO.

THESE pieces are intended to enable one to adjust with accuracy and ease the mass of air to be experimented upon.

V is an iron cistern into which the open or pressure tube O, the closed tube C, and the reversible air-syringe S are screwed air-tight, and the cistern nearly filled with mercury. The syringe being connected for exhausting, and operated, air is withdrawn from C, until the mercury sinks to the bottom of the open tube, when air escapes from it, and rises through the mercury. No more air can be withdrawn from C. The mass of air remaining in C will evidently depend on the difference in depth of immersion of C and O. Let d = this difference, and let it be required to find such a value of d as will permit just enough air to remain in C to fill it from the zero of the scale, when at atmospheric pressure H. Let L = length of C from top to zero, and let l' = the length from zero to the open end of C. If now the mass of air which

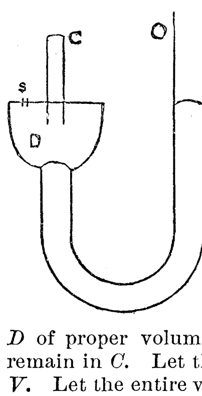


will fill the length l at H be expanded to fill the length l', the pressure H' at the bottom of C by Boyle's law is $H' = \frac{Hl}{l+l'}$.

The pressure at the open end of $O = H$. The difference in pressure at the ends of C and O is that due to a column (d) of mercury. Hence $H' = H - d$. Equating, $H - d = \frac{Hl}{l+l'}$. $\therefore d = \frac{Hl'}{l+l'}$.

On reversing the syringe, and forcing air in, the mercury will be found to rise and stand at zero in both tubes together. The demonstration is continued by forcing in more air.

A second form consists of two glass tubes connected by a strong rubber tube, and mounted on a stand with scales. The closed tube C is sealed into the screw-cover of an iron cistern D. Mercury being poured in, it will expel the air in D, and rise in an open screw-hole S in the cover. The hole being sealed by insertion of the screw, and O lowered, the air in C expands, filling C and D. On raising O, the mercury rises, and cuts off communication between C and D, preventing the return of some of the air. By making D of proper volume, the desired mass of air will remain in C. Let the volume of C above the zero = V. Let the entire volume of $C = V'$, and the volume of D above the open end of $C = V''$. Following the above steps it will be seen that a volume V' at H becomes a volume $V' + V''$ at H'; also that a volume V at H becomes a volume V' at H'. Hence the proportions $V : V' :: V' : V' + V''$, $\therefore V'' = (V' - V)\frac{V'}{V}$. The use of the rubber tube is not new: the method of adjusting the air-mass is be-



lieved to be. This form is convenient because it answers for demonstrating the law at pressures above and below one atmosphere. Of course, for the latter, the air in D must be removed through S as at first.

Natural snow-balls or snow-rollers.

BY SAMUEL HART OF HARTFORD, CONN.

The author carefully described the production and appearance of snow-balls or snow-rollers, the result of natural causes, of which a fine example was presented in Connecticut and southern Massachusetts last February. Snow had fallen, and had been covered with a frozen surface by a light subsequent rain. Upon this surface fresh snow fell, and this under the influence of wind was collected in masses of differing shapes. Some were spherical, from one to nine inches in diameter; most were rollers shaped like muffs, cylindrical, with a conical depression at each end reaching nearly to the middle. The largest observed by the author was 18 inches long, and 12 inches in diameter; but some were reported much larger. The path of formation showed that the roller had started with a small pellet, and, gaining both in length and diameter, had rolled up a long isosceles triangle of snow from its vertex. These paths were observed of a length of 25 to 30 feet, and others were reported as of 60 feet. The paths of the round balls were of nearly the same width throughout. None of these masses could be lifted without breakage. Such rollers were seen over an area of 40 miles in length: the author believed that they must have been millions in number.

Remarks on the tracings of self-registering instruments, and the value of the signal-service indications for Iowa, in June and July, 1883.

BY GUSTAVUS HINRICHS OF IOWA CITY, IO.

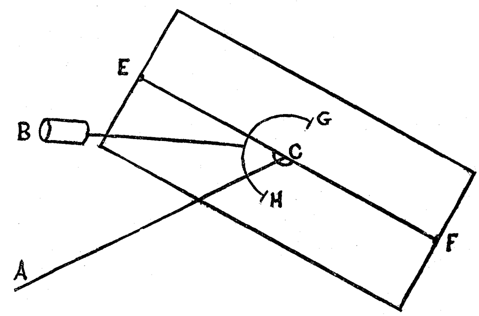
This paper was mainly a severe criticism on the work of the signal-service bureau. The author claimed that the predictions of the weather for Iowa in June had been quite untrustworthy, only 50 per cent proving correct. His views as to the value of this service were vigorously combated in a discussion which followed the reading of his paper.

A new heliostat.

BY B. F. THOMAS OF COLUMBIA, MO.

The instrument is intended to throw the reflected ray horizontally in the meridian.

A polar axis $A\ C$, driven by clockwork, is provided with a declination-arm $E\ F$, with arc, pivoted at C, and pivoted to the longer axis of the mirror at E and F. A 'horizontal axis' $B\ G\ H$, with the shaft portion B supported (by proper bearings in the standard not represented) in the same vertical plane with the polar axis, is pivoted to the mirror in its shorter axis at G and H, the points $E\ C\ F\ G\ H$ being always in one plane, and G and H so adjusted that a line through them will pass through C perpendicular to $E\ C\ F$. Moreover, the axis of the shaft portion B, extended, is perpendicular to and bisects $G\ C\ H$ at C. In order that the reflected ray be thrown horizontally in the meridian by our mirror, two conditions must be met: viz., 1°. The longer axis of the mirror must make and maintain the proper angle between itself and a line from C to the sun; or, what is



the same thing, the angle $A\ C\ E$ must equal latitude plus one-half the sun's altitude at noon. 2°. The shorter axis must move in a vertical plane, perpendicular to the meridian; or, in other words, the prime vertical. These conditions are rigidly met by the above combination.

The static telephone.

BY PROF. A. E. DOLBEAR OF COLLEGE HILL, MASS.

IN the static telephone, a ring of hard rubber is used, within which are two parallel metal-plates separated by a body of air (a non-conductor), one plate connected with the line carrying the current. The electrifying of one plate then causes attraction or repulsion of the free plate, and thus a sound in the receiver. This does away with magnetism. This system therefore requires a very large electro-motive power, and uses an induction coil of 2,000 ohms. A ground or return circuit is not present here. The equivalent is the body itself. There is no passage of electricity from plate to plate: the action is purely inductive through space. The insulation is accomplished by the intervening air-space, and by a coating of varnish, — an excellent di-electric. There is a device to discharge the induction plate in connection with this instrument, which keeps it constantly up to its full possibilities. When this instrument is fully charged, and the electrical conditions are perfect, the receiver may be entirely disconnected from the transmitter, and sounds and conversation can still be heard, even across a room.

He also called attention to the fact, that instruments that have been in use work much better than new ones, as each plate acts as a condenser.

In the discussion which followed, Dr. Dolbear was asked if the state of the atmosphere, in any way, affects the operation of the instrument. I have used these instruments, said he, on an actual line between Boston and New York, in a night when it rained over the whole length of the line, and the whole line was

as badly insulated as it well could be. I have also used it on the same line under the most favorable conditions for insulation, and could not really perceive much difference. It seemed to be as loud at one time as at another.

Pres. H. A. ROWLAND. — Of course this is on an entirely different principle from our telephone. What interested me considerably was the fact, that one could hear better when the plates were charged. The explanation theoretically is very simple, and it is the same as that the Thompson electrometer is more sensitive when the jar is charged than when it is not charged; the reason being, that the attraction is proportionate to the square of the difference of the potentials, rather than the simple difference of the potentials. Therefore a small difference in the quantity, when it is large, produces a greater effect than when it is small. So the explanation is exactly the same as that the Thompson electrometer is more sensitive when you have the jar charged than when you do not. So, the higher the charge one would get, the more sensitive the instrument would be. I was especially interested in it, because it was on such an entirely different principle from the Bell instrument. I don't wish to say any thing about patent laws or decisions on this subject, for they have nothing to do with this; but, scientifically, this is an entirely different instrument from the Bell instrument, and I am especially interested on that account.

Prof. T. C. MENDENHALL. — I profess not to have quite understood the statement made by Professor Dolbear. I should like to hear your own (the president's) opinion with regard to that charge which remains in spite of the fact that the two poles of the condenser are connected by conductors. I may have misunderstood the statement; but if that is correct, I should like to know whether that can be explained or not.

President ROWLAND. — Well, I suppose we all know how retentive an electroscope is of a charge. I suppose the idea is very similar in this case. I do not suppose the plates have a difference of potential. If you should leave them for a moment, I should suppose they would soon have a little return charge. If the two plates of the condenser were together, they would have the same potential. I understood it as merely a return charge. I do not know how Professor Dolbear understands it.

Professor DOLBEAR. — The instrument itself is a most delicate electrometer when tested in this way; and when it is charged and really in good working order, the gentlest tap upon the instrument serves to show that it is in good working order, for one can apply the instrument to his ear and hear himself talk. This is the case, even when the two plates of the condenser are connected with each other through the induction coil; and so, although they may have been there for hours, or even for days, — the difference between an instrument that has not been used and one that has been charged is very appreciable.

President ROWLAND. — I suppose in that case it would be simply from the charge of the varnished surface?

Professor DOLBEAR. — Yes: I think they retain their charge for a much longer time if the surface is varnished. I do think there is a difference between the behavior of this and the charged cable. If a cable be charged for half an hour by battery, it will require half an hour to run out again, but it will be at that time quite discharged. But that is not the case with this instrument.

President ROWLAND. — I should suppose it was the charge in the varnished surface.

Prof. W. A. ANTHONY. — Professor Dolbear did not say any thing about one advantage that this telephone has over the other, that struck me when I read the descriptions of it earlier, — that, in consequence of using this electricity at such a high potential, the ordinary telegraph-lines or other instruments would have very little effect upon it: therefore the telephone is very free from induction.

Professor DOLBEAR. — My experience has been in accordance with that theory. Electro-motive force from induction from telegraph-lines is ordinarily tolerably small, although there may be at times considerable strength of current. But, the electro-motive force being so strong in my circuit, it follows that the action of such induced currents is very slight, and does not interfere with work.

Prof. C. A. YOUNG. — I would like to inquire whether you have tried any experiments in putting the end of the wire to the ear to illustrate the sensitiveness of the ear?

Professor DOLBEAR. — Yes: I have heard simply by putting the end of an insulated wire to my ear, and listening. I consider the instrument as simply the enlarged terminal of a wire, and that you are actually listening at the end of a wire.

Mr. E. GRAY. — I have made a good many experiments in another line, which I may state briefly, which may throw some light upon this, and yet I think it is very well understood. You remember, some of you, reading of such experiments made in 1874, relating to the reproducing of music on a plate by simply rapping with the finger or with some animal tissue. Now, I made this experiment, which seems to prove to my mind that the operation is as Professor Dolbear has explained it. I set my revolving disc, which was a simple disc of zinc, revolving at a steady rate, giving it a pressure with the fingers. Then I had fifty cells of battery set up, as much as I could bear, passing through them, and had some one close the circuit with a Morse key. At the same time the key was closed, my finger would be jerked forward in the direction of the rotation of the disc; and it would remain in that forward condition, showing an increase of friction, until the key would be opened, and then it would drop back; showing that from some cause there was an increase of friction, either due to molecular disturbance, or, what is probably the case, to attraction between the finger and the plate. It is necessary, to produce this experiment, that the cuticle be perfectly dry. You must rub it a long time, and have it perfectly polished; and then the cuticle becomes a dielectric, and the body is charged with one kind of electricity, and the wire or

the plate with another. Later I got some fairly good results in articulation by using a small diaphragm with all the conditions as nearly right as possible; and, having a current of sufficient electro-motive force, I could actually understand words produced on the end of my finger.

President Rowland. — What is the difference between that and Edison's motorphone?

Mr. Gray. — In Edison's motorphone, when the current was thrown on, there was a decrease of friction; there was chemical action taking place on the surface. In this case there is none, and there is an increase of friction when the current is on: perhaps 'current' is a bad word to use.

President Rowland. — The principle is the same.

Mr. Gray. — One is a chemical action, which causes the friction to be less at the moment of charge. In this case, however, this is purely static contact, and increases the friction in the same manner that the plates are thrown together when they are charged in this telephone. And the motion, of course, or sound, is produced by a letting-go of the finger from the plate, and not by actual vibration, in the same sense that it takes place between the two plates in this receiver of Professor Dolbear.

President Rowland. — You attribute it to attraction?

Mr. Gray. — Yes: my experiments seem to prove that; I presume, because there was adhesion, there was an increase of friction during the time of the charge and the letting-go, when the circuit was open. There was really no circuit except when the charge was taken off.

Sec. F. E. Nipher. — In regard to the case of which Professor Dolbear spoke, when it might be supposed that electricity does actually pass from the line into the ground, it seems to me that that fact, so far as it did exist, would be prejudicial to the action of the instruments; that what we want to bring about is not a current, but as great a difference of potential as possible, between the plates.

List of other papers.

The following additional paper was read in this section: — An extension of the theorem of the virial to rotary oscillation, by *H. T. Eddy*.

PROCEEDINGS OF SECTION C. — CHEMISTRY.

Report of the committee on indexing the literature of chemical elements.

The undersigned, a committee appointed at the Montreal meeting of the American association for the advancement of science, "to devise and inaugurate a plan for the proper indexing of the literature of the chemical elements," respectfully submit the following report.

The members have conferred with each other orally and by correspondence. Several plans have been suggested, and their merits discussed. Three methods of collecting material for the indexes may be named: —

1°. Revising the Catalogue of scientific papers published by the Royal Society (8 vols. 4to).

2°. Indexing special journals by different individuals, and collating the matter.

3°. The independent plan, whereby each chemist indexes all the journals available to him with reference to a given element, in which he is presumably especially interested.

Each of these schemes is open to objections, and has its difficulties. The first would necessitate an enormous amount of clerical labor, for which volunteers would scarcely be secured; besides, data previous to 1800 could not be obtained from this catalogue.

The second involves, also, securing a large number of self-sacrificing volunteers; and both plans would require a vast amount of editorial work on the part of this committee.

The third plan seems, to a majority of the committee, the only feasible one at present. On the independent plan, seven indexes have already been compiled. The best arrangement of material has also been considered; and here again a threefold problem occurs: —

1. Chronologically.
2. Alphabetically, by authors.
3. Topically.

The committee do not venture to dictate to independent workers, but recommend the chronological arrangement, with the understanding that a topical index accompany each monograph.

The best channel of publication has also been considered by the committee. All the indexes hitherto published have been printed in the annals of the New-York academy of sciences; and the academy has generously offered, through its officers, to continue its good work. The Smithsonian institution further agrees to distribute, free of expense, all circulars and documents in furtherance of this undertaking; an offer which is of greatest importance, and for which this committee expresses sincere thanks.

Since the appointment of the committee, Mr. Webb's index to the literature of electrolysis has been published in the annals of the New-York academy of sciences; and several chemists have expressed a willingness to co-operate in the proposed undertaking. Prof. R. B. Warder of Cincinnati has promised an index to the literature of the velocity of chemical reactions; and Dr. Henry Leffmann of Philadelphia proposes to index the important element arsenic.

Your committee present to the association this brief report of progress, and respectfully desire to be continued.

H. C. Bolton, *Chairman;* Ira Remsen; F. W. Clarke; A. R. Leeds; A. A. Julien.

PAPERS READ BEFORE SECTION C.

On γ-dichlordibrompropionic and γ-dichlorbromacrylic acids.

BY C. F. MABERY AND H. H. NICHOLSON.

When dry chlorine is passed through β-dibromacrylic acid, the reaction is easily accomplished, and the product may be purified without difficulty by crystallization from carbonic disulphide. This acid is very sparingly soluble in water, more soluble in hot than in cold carbonic disulphide and chloroform. It melts at 100°. Its salts were carefully studied, but the silver salt was found so unstable that it could not be prepared in a state of purity. Since β-dibromacrylic acid has, without doubt, the form

$$\begin{array}{l} CBr_2 \\ \| \\ CH \\ | \\ COOH \end{array}$$

, the chlorine addition product would have the form

$$\begin{array}{l} CBr_2Cl \\ | \\ CHCl \\ | \\ COOH \end{array}$$

. This acid is entirely decomposed when heated with an excess of any alkaline hydrate. If, however, the reaction is allowed to progress in the cold, keeping the hydrate in slight excess, the elements of hydrobromic acid are easily removed, with the formation of the corresponding dichlordibromacrylic acid. In order to distinguish this from two other products which have already been obtained, it will be called the γ-acid. It is prepared by the action of baric hydrate upon γ-dichlordibrompropionic acid, and the reaction proceeds so rapidly that it is difficult to keep the solution alkaline. Upon acidifying the baric hydrate solution with hydrochloric acid, γ-dichlorbromacrylic acid is precipitated partly as a crystalline solid, and is easily purified by crystallization from hot water. It is sparingly soluble in cold, readily in hot water, and in alcohol, ether, carbonic disulphide, and chloroform. It crystallizes in pearly-white scales, which melt at 78° to 80°. For further identification, the acid was analyzed, and its salts submitted to careful study.

The sub-aqueous dissociation of certain salts.

BY JOHN W. LANGLEY AND CHARLES K. McGEE OF ANN ARBOR, MICH.

The question as to whether salts are dissociated into their components when simply dissolved in water has been attacked by different chemists in various ways. The method described in this paper seems to have furnished some remarkable results, which may help in pointing the way to the final answer of this important problem.

Sainte Claire Deville has called attention to apparent chemical changes which a salt may undergo by the mere fact of solution, and that such changes may increase in extent with the mere addition of water. He concludes that there is no absolute distinction between solution and chemical union; that the difference is rather of degree than of kind. From this point of view, a salt in dissolving has its particles separated much as if it were vaporized by heat, and the heat units necessary to perform this sort of vaporization are taken from surrounding bodies. As the heat absorbed increases with the degree of dilution, it will eventually become sufficient to dissociate into its elements a salt dissolved in a suitably large quantity of a neutral solvent, such as water.

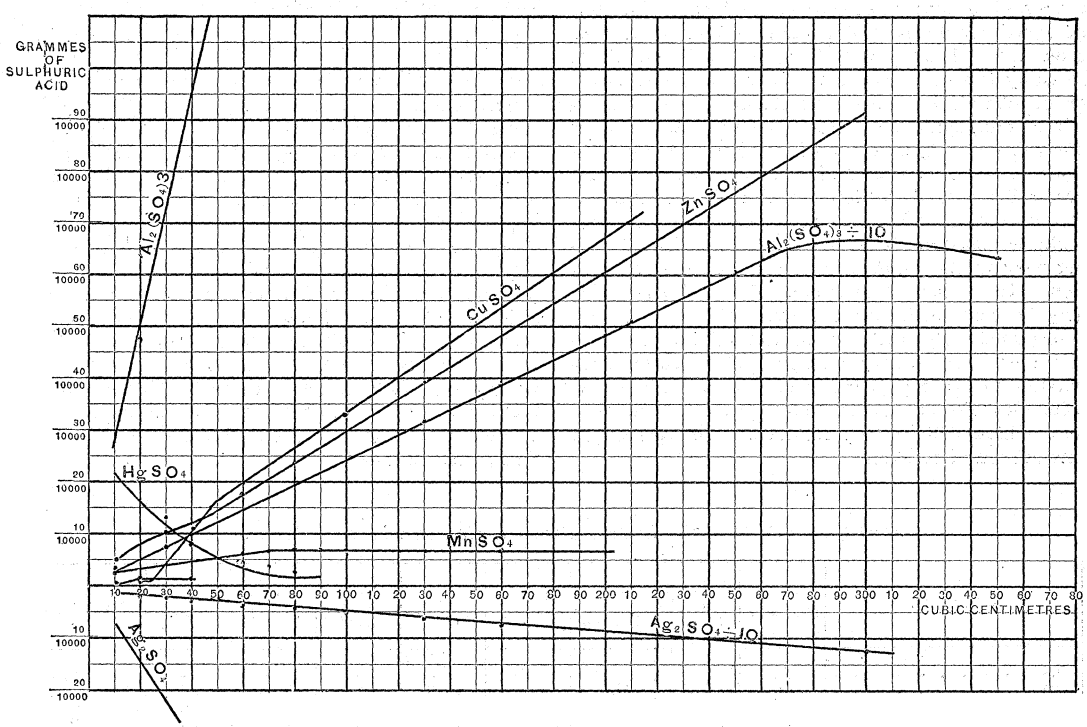
Assuming that salts tend to dissociate by solution, and are decomposed when sufficiently diluted, we should expect them to break into simpler molecules first, and, of course, along the lines of least resistance. We may take three views of the possible condition of a salt dissolved in a small quantity of water — as, for instance, one molecule of sodic sulphate in two molecules of water: 1. That it is attached to the water by a sort of physical adhesion, which may be represented by $[Na_2\,SO_4\ ,\ 2H_2\,O]$. 2. That the water and salt are united in a new group which acts as a compound molecule so long as the amount of the solvent is small; this might be $[2\ (Na\,OH)\ ,\ H_2\,SO_4]$, the comma indicating a molecular as distinguished from an atomic union. 3. That we have in these cases a certain quantity of different kinds of matter held momentarily in equilibrium, but ready to form definite combinations when the external forces change. The last view would be expressed by $[Na_2\,H_2\,SO_6]$, and does not require that Na be combined with H, S, or SO_4.

The heat of combination between $H_2\,SO_4$ and $2Na\,OH$ is less than that in the formation of sodium hydrate starting with metallic sodium and water, or of sulphuric acid starting with SO_3 and water. Therefore, in the group $Na_2\,H_2\,SO_6$, the line of least resistance probably passes through where the comma is placed in the arrangement $[2\ (Na\,OH)\ ,\ H_2\,SO_4]$. Then the first stage of dissociation will be the appearance of free sodium hydrate and free sulphuric acid. The change will be partial for finite ratios between quantities of the salt and the water, and should gradually increase with augmented dilution to a point where free acid can be shown quantitatively.

For the present occasion, advantage was taken of the circumstance that in some neutral salts the bases have less power to turn litmus blue than the acids have to turn it red; and also, that in certain other salts the converse is true. Thus the power of the hydrates of zinc, iron, and copper, to turn litmus blue, is quite feeble; while the power of the mineral acids to redden litmus is very great. On the other hand, the hydrates of the alkaline metals are singularly powerful in turning litmus blue. Now, if the power of the base to produce the blue is not the exact quantitative counterpart of the acid to produce red, the difference of color-producing power must increase in proportion as the solution becomes more dilute, if the theory of dissociation is well founded.

The method of experiment may be briefly stated. A series of test tubes was prepared, holding respectively one, two, three, four, etc., portions of sulphuric acid; and each was then diluted with litmus solution to an equal amount. The tubes thus filled

presented a series from neutral purple to decided red. This formed a scale of colors for reference. Saturated solutions were prepared of the sulphates of Zn, Cu, Pb, Ag, Ca, Na, Hg, Mn, Al, and Fe, and Zn Cl_2. To each of these solutions, enough litmus solution was added, in a series for each salt, to exactly correspond in amount with the sulphuric-acid tubes. As each tube of a dissolved salt was prepared, and also as it was successively diluted with increasing amounts of litmus, it was compared with the tubes in the color-scale, by looking across the two tubes, until its corresponding tint was found. A complete record of these correspondences was made; and it furnished the means for constructing a diagram, in which the results are plotted in curves.



The following were the chief results: Ca So_4 and Na_2 So_4 each continued to act as a neutral salt, without effect on the litmus throughout the range of dilutions. $Ag_2 SO_4$ was the only salt which changed the solution to a blue. The results with $Fe_2 (SO_4)_3$ and Fe SO_4 were unsatisfactory because of a dirty precipitate, but both made the litmus red. Zn Cl_2 presented a similar difficulty. There is a doubt about the result with Hg SO_4, and some obscurity about the greatly diluted solutions of $Al_2 (SO_4)_3$ and Cu So_4.

On account of instability of color, probably caused by oxidation, a fresh color-scale had to be prepared every day, and the mixtures were made under a film of paraffine.

These experiments seem to indicate that: 1°. Sulphates of the alkali metals, except silver, are strictly neutral under all degrees of dilution. 2°. Sulphates of the R. SO_4 type, where R is a dyad metal, show an amount of dissociation proportioned to the degree of dilution. 3°. Aluminic sulphate, and other double triads, are not neutral when concentrated. When diluted they soon become strongly acid. When the dilution exceeds a certain limit, they lose acid at a decreasing rate.

Suggestions for computing the speed of chemical reactions.

BY R. B. WARDER OF CINCINNATI, O.

THIS paper urges a thorough discussion of data upon the subject indicated in its title, for the following reasons: 1°. To discover and investigate the causes of certain discrepancies between published observations and current theories. 2°. To obtain more definite information as to the nature of certain reactions and the conditions determining their speed. 3°. To afford numerical data for a fuller study of relations between the speed of reactions and other physical constants. 4°. To suggest fruitful lines for further research in chemical dynamics.

As instances of the need of such discussion, the determinations by Professor Menschutkin, of the speed and limits of the etherification of the several alcohols and acids, give numbers for the initial speed of reaction in one hour which are not proportional to speeds during the first minute. Prof. L. Meyer in his *Dynamik der atomen* passes very lightly over both the theory and the observations of speed during a reaction.

The prevalent theory of the action of mass is expressed, $\frac{du}{dt} = kuv \ldots$ in which the differential expresses the rate of change in any substance, u and v represent the masses taking part in the change, and k is a constant. Some observations by Ostwald and others indicate that some modifications of this theory are needed. Determinations of the speed of reaction require special care, both to measure time in relation to mass, and to control temperature and other conditions. The chemical section of the Ohio mechanics' institute has recently undertaken some work of the sort, and invites co-operation.

The following provisional system is suggested: for volume, one cc.; for mass, the chemical equivalent expressed in mg.; and for time, one hour. The unit of speed would be the transformation of unit of each active body per unit of volume and time. Possibly the comparison of the constants of speed or of chemical affinity with those of heat, electricity, etc., could be better made from the unit of one second or 1,000 seconds. At least two observations of time and two of mass are required, and preferably several, to determine the limits of error. Determinations which do not accord with the hypothesis that diminished speed and diminished product vary in the same ratio, need special investigation. In reciprocal reactions, some of the ratios may be combined with constants of speed already determined. By bringing all the facts into systematic order, these data can be made of use for comparison in other physical-science fields. The paper concludes with an extended bibliography of the subject, which will be very serviceable to workers in this branch of research.

Twelve months of lysimeter record at the New-York agricultural experiment station.

BY E. L. STURTEVANT OF GENEVA, N.Y.

THE lysimeters were described. They are boxes of peculiar construction, containing selected samples of soils in layers. The relative percolation of rainfall through these different soils, and the evaporation, are determined by observations of the instrument. The results are summarized as follows: Sod land allowed 11.68 of the rainfall to percolate; soil of which the surface was simply bared allowed 25.88 per cent percolation; the cultivated soil passed 37.93 per cent. The evaporation from the first of these was, of course, 88.32 per cent; from the second, 74.12; from the third, 62.07; the sum of percolation and evaporation being held to account for the entire rain-fall.

The composition of American wheat and corn.

BY CLIFFORD RICHARDSON OF WASHINGTON, D.C.

THIS paper gave an account of results obtained by the author in his work as first assistant chemist of the U. S. department of agriculture. More than 200 analyses of wheat, and 100 of corn, have been made during the last ten years under his supervision. It appears that while our wheats are of somewhat lighter weight, they contain less water, about the same ash, more oil, less fibre, and less albumen, than the foreign wheats. Among our wheats, only those from Colorado, Dakota, and Minnesota equal the European in albuminoids and in size of grain. The wheats of the Atlantic states are poor in nitrogen. Corn, compared with wheat, contains twice as much oil, less starch, more water and fibre, and less of albuminoids. The following table gives a condensed statement of the wheat analyses: —

Average percentage of nitrogen, albumen, etc., in wheats of the world.

COUNTRIES.	No. of analyses.	Per cent of nitrogen.	Per cent of albumen.	Highest albumen.	Lowest albumen.	Weight of 100 kernels.	Highest weight.	Lowest weight.	Authority.
Russia	24	3.12	19.48	24.56	10.68	–	–	–	Laskowsky.
Russia	5	2.34	14.63	16.56	14.24	3.610	5.350	2.000	Von Bibra.
North Germany	25	2.24	14.00	18.26	9.80	4.498	5.400	4.000	" "
South Germany	13	2.17	13.56	17.76	10.21	4.485	7.000	2.875	" "
Germany	–	2.11	13.19	–	–	–	–	–	Kühn.
Germany	–	2.08	13.09	–	–	–	–	–	Wolff.
Spain	8	2.10	13.13	15.29	11.26	4.270	5.125	3.275	Von Bibra.
France	–	2.08	13.00	–	–	–	–	–	Reiset.
Scotland	14	2.01	12.56	–	–	4.680	5.200	4.250	Von Bibra.
Australia	2	1.60	10.00	–	–	–	–	–	" "
Egypt	5	1.47	9.19	9.92	8.75	5.540	–	–	" "
All but Russia	176	2.29	13.65	19.10	5.33	–	–	–	Koenig.
America	254	1.92	12.00	17.15	8.05	–	5.924	1.830	Various.
America, except Colorado	163	1.86	11.62	16.63	8.05	3.532	5.079	1.830	"
Colorado, 1881	33	2.14	13.40	15.94	11.19	4.833	5.924	3.851	Richardson.
Colorado, 1882	12	2.09	13.06	14.88	11.55	4.299	4.670	3.976	"
Minnesota	12	2.05	12.79	17.15	10.85	3.354	3.699	3.116	"
Michigan	38	1.92	12.00	14.47	9.13	4.116	–	–	Kedzie.
Missouri	10	1.83	11.44	12.44	10.50	3.502	3.867	3.098	Richardson.
Oregon	7	1.46	9.17	10.63	8.05	4.800	–	–	"
Atlantic States	56	1.79	11.18	14.00	8.93	3.057	4.628	1.830	"
Pennsylvania	23	1.80	11.25	12.78	9.45	3.211	4.063	2.526	"
North Carolina	21	1.67	10.46	12.43	8.93	3.782	4.628	2.780	"
Alabama	17	1.82	11.32	13.65	9.80	3.137	4.647	2.011	"

The sotol, a Mexican forage plant.

BY CLIFFORD RICHARDSON OF WASHINGTON, D.C.

THIS plant, Dasylinion texanum, grows wild and extensively on the borders of the Rio Grande and elsewhere in Texas, and in Mexico, on a rocky and gravelly soil. The plains covered with it look like a vast cabbage-field. Sheep feeding on it go without water for many weeks. Only the bulb is eaten. It is split open by the shepherd, who carries a knife for the purpose. Mexicans eat the bulb after roasting or baking it in pits. Also a liquor is obtained from it, by fermenting and distilling after roasting, called 'sotol mescal,' and possessed of highly intoxicating powers.

The plant is described in Watson's Revision of the North-American Liliaceae. About 18 per cent of sugar can be obtained from the outer husks; in the interior, more than 10.5 per cent exists; and in the whole head of the plant there is probably more than 15.5 per cent of sugars. No starch seems to be present.

A proximate analysis of the soft interior of the head gave 17 per cent sugars: 65 per cent of this soft substance in the head, when fresh, is water.

As a food-plant in dry districts, the sotol is of great value; as a fibre-producing plant, it will not be of any importance, owing to the shortness of the cells.

American butters and their adulterations.

BY H. W. WILEY OF WASHINGTON, D.C.

A SERIES of elaborate experiments and analyses of various samples of butter, oleomargarine, tallow, and lard, have been made by Professor Wiley, chemist of the U.S. department of agriculture. The paper contained a description of Professor Wiley's method. He takes a weighed quantity of the butter, puts it in a sand-bath, and dries for two hours at 100°. The curd or caseine is determined by ignition: five grains are used for the purpose. Dry combustion in a tube is difficult and unsatisfactory: he therefore uses the moist-combustion method, with permanganate and nesslerizing. The amount of salt he considers important. It is usually determined by ignition, and weighing the residue; but he found that so much chlorine was thereby lost, that the result was not trustworthy. He washes the butter by shaking it in a separating funnel with hot water, and then determines the chlorine with standard silver nitrate and potassium bichromate as an indicator.

Professor Wiley has devised several novelties for these analyses. One of the neatest is for ascertaining the melting-point. The butter is packed in a U-shaped tube, of which one leg is longer than the other. The tube is placed upright in a vessel containing sufficient mercury to overflow the top of the tube. This vessel is placed in another containing water, and heat is applied beneath. The water, heated, in turn heats the mercury surrounding the tube, until the contents of the tube are melted. As soon as the melting takes place, the melted material leaves the tube, and floats on the surface of the mercury. Another method consisted in laying platinum wires upon the sample of butter, etc., heating the wires, and noting the heat required to cause them to disappear by sinking into the sample. These methods determined not only the melting-point of samples of butter, oleomargarine, tallow, and lard, but also of the fatty acids. But the variations in the melting-point of genuine butter are so wide, that no certain conclusion can be arrived at by comparison with melting-point of oleomargarine, etc., to test the question of genuineness. Thus it was found that first-rate butter from an Alderney cow at one time, owing to special feeding, had a higher melting-point than oleomargarine; while a few weeks later, with different food, the same cow supplied milk from which was made butter with a lower melting-point than oleomargarine.

In regard to other tests, concerning which full details were given in the paper, it may be briefly stated, that, as a general rule, the amount of caseine present in pure butter is much greater than in oleomargarine. The specific gravity of genuine butter is lower. The saturation co-efficient for the insoluble acids in the genuine butter is low, in the imitations it is high. Professor Wiley seems to place more reliance on tests for saturation co-efficient than on other methods. The soluble fatty acids in pure butter range from three to five per cent; while in oleomargarine, tallow, etc., they are either absent, or show a mere trace. The author also called attention to polarization tests. The genuine butter gives a uniform field in polarized light: oleomargarine gives a field with mottled and crystalline structure. He had made no analysis of butter known or suspected to be adulterated by mixture. He considered it unwise to decide the question of genuineness from any one of the constituents or conditions of a sample; believing that all the different tests should be brought to bear. He presented elaborate tables of analyses of different kinds of butter, etc.; specifying for each the place of purchase, name sold by, price, color, percentage of water, of caseine, of salt, specific gravity at 40°, melting and solidifying points, percentage of soluble and of insoluble acids, and the melting and solidifying points and the saturation equivalent of the insoluble acids.

The discussion respecting the analysis of butter which was brought about by this paper revolved around the question of the value of the data presented for the practical work of the determination of actual proportion of adulteration. Mr. Noyes held that the variations in pure butters in specific gravity, in melting-point, in saturation co-efficient, and in caseine, as determined by Professor Wiley, would be of little value except in cases where the adulteration was very great. Mr. Springer held that the principal constituent to be taken into account in the determination of adulteration was the amount of caseine, and that although there were some difficulties in the way of its accurate determination, they might be removed, and he should then have more faith in this than in the comparison of other data. He suggested that the

accurate determination of caseine might be effected by some rapid-fermentation process by which caseine could be broken up into other organic products that could be separated by albumen. He held to this point as to caseine, because it cannot conveniently be added in the manufacture of oleomargarine; while the acids upon which the saturation co-efficient depends could readily be added as sodium compounds.

On account of the difficulty of getting accurate results in determining nitrogen, it was thought best to use the wet-combustion method with permanganate, because a small quantity of material might be used, and there would be fewer chances for loss that otherwise occurs in nitrogen determinations that are effected by the combustion of butters.

Dr. Wheeler called the attention of the section to the use of what is known as 'cotton-seed-oil stock,' in the manufacture of oleomargarine. This, doubtless, contains considerable nitrogen, and, of course, would reduce the value of the caseine-test for adulteration. A sample was shown, supposed to contain cotton-seed-oil.

The sense of the discussion was, that it was very desirable that Professor Wiley should continue his experiments, as they are of great value; but there is yet a great deal of work to be done in the investigation.

List of other papers.

The following additional papers were read in this section:—The formation and constitution of chlordibromacrylic acid, by *C. F. Mabery* and *Rachel Lloyd*. Orthiodtoluolsulfonic acid, by *C. F. Mabery* and *G. M. Palmer*. Estimation of carbon and nitrogen in organic compounds, by *C. Leo Mees*. New forms of burettes, by *W. H. Seaman*.

PROCEEDINGS OF SECTION D.—MECHANICAL SCIENCE.

PAPERS READ BEFORE SECTION D.

A comparison of terra-cotta lumber with other materials.

BY T. R. BAKER OF MILLERSVILLE, PENN.

THE material called 'terra-cotta lumber' is made out of clay and sawdust. The investigation which formed the subject of this paper was to ascertain certain qualities of this artificial product. The paper also described the apparatus used for the tests. The results indicated that the material was 875 times as permeable to air as pine, and 135 times as brick. Air was forced by pressure of a column of water. Other tests showed that the material was four times as hard as pine, but not so hard as brick. Its grip on nails driven into it was about half that of pine. The author was careful to disclaim any intention of advertising the merits of the material, but he evidently regarded it as serviceable for the purposes for which it is intended. Specimens were exhibited.

Improvements in shaping-machines.

BY J. BURKITT WEBB OF ITHACA, N.Y.

IN the ordinary shaping-machine there are two defects, one of which is found also in the planer. The ram of a shaping-machine is a bar sliding in bearings, and carrying at one end the cutting-tool. If we represent by a the variable horizontal distance from the tool to the first bearing (or nearest end of the long bearing), and by b the variable horizontal distance from the tool to the second or farthest bearing (back end of long bearing), and by c the length of stroke, we shall have,—

Maximum value of $a =$ (minimum value of a) $+ c$.
Maximum value of $b =$ (minimum value of b) $+ c$.

In other words, the length of the ram is variable, and the spring of the ram from the work is variable, the tool springing away from its work more at the end of its stroke. This springing takes place mostly in the joint between the ram and its bearings, and cannot be wholly avoided without a change of construction. To remedy the defect, the author proposes a reversed construction of the sliding parts; the two bearings (preferable to a long bearing) to be formed on the ram, so as to make the distances a and b constant, and the long slide being part of the bed of the machine.

The second defect, which is also common to the planer, is in having a 'drop-block' which fits but indifferently between the jaws and against the bottom of its seat. From the necessity of the usual construction, the tool attached to this block will have more or less spring. The remedy is to dispense with the drop-block, and introduce an automatic motion to lift the tool on the return stroke, as has been done, the author has understood, on some large machines.

Regularity of flow in double-cylinder rotary pumps.

BY J. BURKITT WEBB OF ITHACA, N.Y.

THE speaker introduced his subject by exhibiting a number of models of these pumps from the cabinets of Cornell university, which has recently purchased copies (243 in number) of the celebrated models of the Reuleaux collection in Berlin. Class I. of this collection is devoted to these pumps. The speaker then produced and demonstrated a formula for the flow of these pumps, and showed that the regularity of flow depended upon other principles opposite to those which have been given for determining this point. The formula given for the flow was:—

$\pi[R'^2 + R''^2 - (r'^2 + r''^2)] =$ Flow for one revolution, when R' and R'' (generally equal to each other) are the extreme radii of the two revolving wheels; and r' and r'' are the radii (often, perhaps generally, variable) from the point of contact between the wheels to their centres. It was shown that the regularity of flow depends upon $r'^2 + r''^2 =$ constant. R' and R'' may be called the 'piston radii,' and r' and r'' the 'valve radii.' These pumps are

called by Reuleaux 'Kapsel räderwerke,' or 'chamber-crank trains,' according to Kennedy.

List of other papers.

The following additional papers were read in this section, some of them by title only: — A method of testing long plane surfaces, applicable to the alignment of planer-beds, lathe-beds, heavy shafting, etc., by *W. A. Rogers.* The commercial and dynamic efficiencies of the steam-engine; Centrifugal action in turbines, by *R. H. Thurston.* Velocity of the piston of a crank engine, by *C. M. Woodward.*

PROCEEDINGS OF SECTION E. — GEOLOGY AND GEOGRAPHY.

ADDRESS OF C. H. HITCHCOCK OF HANOVER, N.H., VICE-PRESIDENT OF SECTION E, AUG. 15, 1883.

THE EARLY HISTORY OF THE NORTH-AMERICAN CONTINENT.

THERE is a special appropriateness in the association of geography with geology, as indicated in the assignment of sciences to section E; for the latter gives us an account of the origin of every topographical feature of the earth's surface, whether island, continent, mountain, plateau, valley, or oceanic depression. If we would properly understand the significance of the earth's contours, we must unravel the mysteries of geology: so a knowledge of topography is essential to the complete comprehension of the geological features of any country. If a geologist were taken by a balloon to an unexplored part of the earth, he would instantly recognize, from their topographical outlines, volcanic and granitic cones, limestone hills, elevated plateaus of basalt or horizontal sandstones, and special types of orographic structure. Hence the modern geologist first draws the contours of his district before applying the colors of geological age. The existing relief features of the earth have been produced one by one in successive periods; and it is the task of the geologist to discover what were the characteristic physical developments of the several ages. He can delineate a connected historical sketch of the beginning, growth, and completion of a continent. Such histories are rare, because attention has but recently been turned into this direction. One of the first American geologists to frame such an outline is Prof. J. D. Dana, to whom we owe the enunciation of this fundamental truth, — that the first formed land has always remained above water, and has been a nucleus about which zones of sediment have accumulated. We can now recognize this primitive continent, with all its successive stages of growth, upon every geological map.

Time would fail us to present the entire physical history of our continent; and we will therefore confine our attention chiefly to its earlier chapters, noting those points which are under discussion. As we are endeavoring to *advance* science, we must touch upon debatable topics, and hope by friendly discussions to become wiser.

We must assume the correctness of the commonly received opinions concerning the earliest history of our planet, — that it passed through the condition of a nebula, and then of a burning sun, the period of igneous fluidity. By subsequent refrigeration it has become either partially or wholly solid. Not until a crust had formed, and the earth had cooled enough to allow water to remain permanently, was it possible to talk of dry land and ocean. With these premises allowed, it seems to us evident that the material of the earth must be disposed in concentric zones, arranged according to density, the heaviest being at the centre. If the various elements were free to move, as is the case in all natural or artificial igneous fluids, we must expect to find the heavier metals situated beneath the others; and, following the analogy of extra-terrestrial bodies, the central nucleus may be principally iron, like the heavier meteors. Zones corresponding to stony meteors, lavas, the trap family, and granites would naturally succeed in order, the last named being at the surface. This outer zone is also characterized by the presence of much silica and oxygen. The primeval ocean came from the vapors surrounding the igneous sphere, condensed to liquidity as soon as water could remain upon the solid crust without immediate vaporization.

This original crust may have been essentially a plain, and consequently entirely covered by water; for if the land were now levelled off, the ocean would submerge every acre of the continents. As refrigeration progressed, ridges and valleys would form in accordance with that fundamental principle that the outer envelope must conform to the shrunken nucleus; and this contraction gives rise to that tangential force or lateral pressure which has acted through all time. Whether these earliest ridges rose above the ocean would depend upon the amount of elevation. Some authors argue that these ridges follow the course of great circles. If there are causes adequate to produce such results, — or any other world-wide arrangement, — they must have commenced to operate at the very beginning of contraction. Most authors maintain that the very thick strata of the older rocks have been formed just like modern sediments, having been broken off the ledges, and transported into oceanic basins in horizontal attitude. If so, there must have been great mountainous elevations, deep oceanic depressions, and extensive aqueous action; since the thickness of the crystalline schists is greater than that of the strata in the fossiliferous ages. The amount of distortion, crumpling, and faulting of the crystalline rocks is also greater. These same authors hold that the original strata were in all respects like modern sands, gravels, and clays, and that their present crystalline structure is due to metamorphism. No one has yet discovered any uncrystalline pre-Cambrian beds; nor

have the original foundation rocks been pointed out, since the oldest known layers are stratified, and cannot therefore have constituted part of the original unstratified crust.

Professor Dana thinks the primitive land originated because of a difference in the rate of conduction of heat during the process of refrigeration. Cooling would be fastest where the heat was conducted most rapidly. The first areas to cool would be the first to solidify. The first solidified crust was heavier than the adjacent liquid: so it sank until it found a fluid as dense as itself. Then the liquid above this crust would in turn become solid, and sink; and this process is supposed to have continued until a permanent shell had become fixed in the earth's circumference, which constantly increased in breadth and thickness, becoming continents. Meanwhile the other portions remained liquid; and their surfaces must have stood at the same level with the first-formed crust till that congealed, and became depressed because of the diminution of volume in solidification. These depressions became the ocean's beds. From this beginning down to the present time the processes of growth have consisted in the thickening of the continents and the settling-down of the oceanic depressions, while the chief force employed has been the lateral pressure derived from contraction. LeConte and Pratt express the process thus far described by the term 'unequal radial contraction.' The total gravity of the particles of matter along each radius is supposed to be the same; and hence, if the heat is conducted most rapidly over the shorter radii composed of denser minerals, the ocean-basins would cool first. These two views thus demand a different arrangement of the lighter and denser materials; the one necessitating that the continents, and the other the depressions, were first to congeal. Both, however, make the gratuitous and unproved assumption, that the surface was not uniform in composition; the differences being probably like that between granite and trap. The principle stated above — that, where all the particles are free to move in a liquid, the lighter elements must rise to the surface, and the heavier minerals sink down in proportion to their specific gravity — is at variance with this assumption. Fortunately it is not essential to a right theory of continental growth. There is no reason, therefore, to doubt that the original crust had essentially a uniform thickness over the whole earth. Contraction would originate ridges and valleys in the normal way, most likely of similar dimensions. There must soon burst forth ejections of igneous matter, owing to tidal attraction; and these would show themselves along the weakest lines. At the outset it is difficult to assign reasons why either the elevations or depressions would be the weaker; and hence we should look for a multitude of locations of igneous overflow, both over the future continents and oceans. There may be no better reason for the eventual enlargement of certain of these volcanoes than that circumstances only very slightly favored them; but, this favor being continued, they would exist and enlarge at the expense of the others, affording us another illustration of the 'survival of the fittest.'

It seems to us there is now afforded an opportunity for reviving in a modified form the view of Poulett Scrope in regard to the origination of the earlier crystalline deposits. Suppose we say, that, besides the original unstratified igneous granitic material, the oldest stratified crystalline rocks are derived from volcanic ejections; being the continued enlargement in size, and reduction in number, of the early indeterminate vents. The several ejections would increase in size till they became islands, either gneissic or granitic; and, if an archipelago is allowed us, we can easily show how continents would accumulate, using only the universally acting forces of lateral pressure and sedimentary accumulation.

Of other theories relating to the origin of the earlier crystalline beds I may mention two. The first is that advocated by Lyell, who termed these rocks *hypogene*. After the solid granitic crust had been formed by refrigeration, "the hot waters of the ocean held in solution the ingredients of gneiss, mica-schist, hornblende-schist, clay slate, and marble, — rocks which were precipitated one after the other in a crystalline form" (Lyell's Principles of geology, 10th ed., i. 142). In such a menstruum, life could not have existed. A very similiar view was advocated by Dr. T. Sterry Hunt in his presidential address before this association in 1871.

The second is the view more commonly entertained, — that, after the solidification of the crust, sedimentation accumulated stratified systems from the granitic foundations, as ordinary sand, gravel, and clay, which were subsequently acted upon by thermal and aqueous influences termed *metamorphic*, and thus converted into crystalline schists. The widespread and powerful action of metamorphism is conceded; but it is a more appropriate adjunct to volcanic than sedimentary accumulation.

A few of the considerations favoring our theory will now be presented.

1. Considering the igneous origin of the earth, volcanic energies would naturally continue their action as soon as there was a crust to be broken through, and immense piles of melted rock would ooze from the numerous fissures. Up to Laurentian times all admit the universality of igneous outflow, while but few have ventured to speak of any thing like volcanic action, except as it has been manifested in the formation of dikes in these early periods. There has been a tendency to class the ancient granites and porphyries with rocks of sedimentary origin, and consequently to restrict the action of igneous agencies to phenomena of slight importance. Several English writers, and, in our country, Dr. Selwyn of Canada, have been calling our attention to the existence of a volcanic group in later Huronian or early Cambrian times. These are the rocks so largely developed about Lake Superior, New England, and the Province of Quebec, consisting of stratified schists, diorites, diabases, amygdaloids, and felsites, identical in composition with true eruptive masses of the same name. Investigation shows that

oftentimes these schists are disposed like the lavas ejected from one series of volcanic vents. Suppose, for example, that Etna or Vesuvius should become extinct. In the course of ages the rains would obliterate the craters, and reduce the lavas to a rounded dome of greater or less regularity. We should recognize the volcanic origin of the mountain in the absence of craters from the lithological similarity of the rocks to those known to have been melted and ejected from vents, while the disposal of the material in a conical attitude shows us that it might once have been covered by craters. So we find in our eastern country many domes of diabasic or protogenic schists, whose volcanic origin may be predicated, both from their lithological character and physical aspect.

Now, this volcanic group of Huronian times indicates the existence of a greater degree of igneous activity than has been described for the paleozoic ages, even those of Great Britain ; and consequently this is an indication pointing significantly towards the predominance of thermal influences in the still earlier periods. In the Laurentian age the fires should have been yet more vigorous, because the time of universal igneous fluidity was less remote.

2. A careful study of the crystalline rocks of the Atlantic slope indicates the presence of scattered ovoidal areas of Laurentian gneisses. Those best known have been described in the Geology of New Hampshire. Instead of a few large synclinal troughs filled to great depths with sediments, the oldest group is disposed in no less than twenty-two areas of small size, scattered like the islands in an archipelago. In a chapter upon the physical history of the state, I have proposed the theory that the earliest land within its limits consisted of this series of islands, not packed as closely together as now, in an area of perhaps three thousand five hundred square miles, but as much more widely separated as would be determined by smoothing out the various anticlinals and synclinals that were formed later. By reference to our maps in Maine, Massachusetts, New Jersey, Pennsylvania, Virginia, North Carolina, and Georgia, many similar ovoidal Laurentian areas may be specified, usually larger than those of New Hampshire. This may be due partly to a less thorough knowledge of the exact areas occupied by this older gneiss, and partly to the existence of a greater number of volcanic vents, giving rise to a more widely spread and thicker mass of ejected material. Over the Atlantic slope and Canadian highlands these primeval islands have, in later periods, been cemented together by a subsequent deposition of material; but in Missouri, Arkansas, and Texas, we recognize, even now, these early islands.

3. The lithology of the Laurentian and other crystalline rocks is very like that of igneous ejections. It is proper at this point to recall the proper restriction of the term Laurentian. As originally defined by Logan, it included every formation that antedated the Huronian. In the Report upon the geology of Canada for 1877–78, Selwyn proposes to restrict the Laurentian outcrops to "all those clearly lower, unconformable, granitoid, or syenitic gneisses in which we never find interstratified bands of calcareous, argillaceous, arenaceous, and conglomeratic rocks." The Hastings and Grenville series, and all the schists containing the eozoon, are excluded from the Laurentian by this definition, as well as the Bethlehem, Lake Winnipiseogee, and Montalban groups of the Atlantic slope. The Laurentian is azoic, the other groups eozoic; and, unless newer distinctions are to be made hereafter, it looks as if we might claim these various azoic Laurentian islands as the first-formed dry land, as they certainly are the nuclei of the existing continents.

There are no minerals in these Laurentian islands that do not occur in eruptive granite; and the schistose structure is often so faint that the field geologist need not be blamed if he acknowledges his inability to detect it. Likewise we discover the same fluidal inclusions and the vacuoles that pertain to granite. If we follow Sorby and Clifton Ward in saying that granite has been formed beneath a pressure equivalent to a weight of forty thousand feet of strata, the same must be said of the early gneisses. With this general assertion of the identity of gneiss and eruptive granite, we must be satisfied at present, without entering into detail.

4. The analogy of the origin of oceanic islands at the present day suggests the igneous derivation of the Laurentian areas. Most of the high islands of the Pacific are composed of lava, with the volcanoes frequently in action. Hawaii, of the Hawaiian group, may illustrate their position and shape. Its area above the water-line is 4,210 □ miles, and its cubical contents above the sea-level are about the same with those of New Hampshire. It rises from a plateau over 16,000 feet deep, thus forming a cone 30,000 feet high, whose cubical contents must be twenty times greater than the portion making dry land. The length of the entire series of islands, all of similar character, is 350 miles, and the area of the base of the lava must be about 100,000 □ miles. These cones have been built up by the accumulation of lava ejected from the interior of the earth, and they are entirely isolated, the nearest land being 1,000 miles distant. The ground-plan of this volcanic mass is that of two elliptical areas, either of which is like some of our Laurentian islands, and is certainly as large as any of these ancient lands south of the St. Lawrence. The land area of the Hawaiian Islands is less than that of Massachusetts, but their base must be equal to the whole of New England and New York combined. Surely it cannot be avowed that volcanic areas are too small to be compared with the space occupied by our oldest formation.

The so-called lowlands are likewise of volcanic origin; since coral polyps have built up reefs upon the igneous area after the disappearance of the fire, and the Hawaiian areas are encircled by reefs. After the volcanoes have become cold, loose material would be worked in between them, coral reefs would grow, and, in various ways, the land area would be enlarged, and finally an archipelago may become a large island. It needs only time and a repetition of these construc-

tive agencies to make a continent out of a series of archipelagos.

There are two points requiring explanation in this connection, — first, the supposed deeply seated localities where granite is produced; and, second, the origin of foliation in the schists.

We should naturally expect that the earlier igneous rocks would have been derived from reservoirs quite near the surface, because of the thinness of the crust. With this notion agrees the presence of cavities containing liquid, and of hydrated minerals, which are more common in the older eruptive rocks, and have led to the aqueo-igneous theories of the origin of granite. Water would be scarce at great depths, and hence these rocks ought to originate near the surface where moisture was abundant. It seems to us that this consideration should more than balance the arguments usually cited in favor of the origin of granite at enormous depths, as it is difficult to see how both can be true.

Mr. H. C. Sorby has led the way in studies of the mineral constituents of eruptive rocks. He measures the included cavities in the component minerals, and calculates how much the contained substances must have contracted in cooling, allowing for an increase in the temperature of the point of vaporization under pressure. By assuming the temperature to be correctly determined, he ascertains the amount of pressure indicated by mathematical formulae, and finds it to be the equivalent of a thickness of 40,000 feet of overlying rock in Cornish granites, and of 60,000 feet in Scotch granites. Later writers seem to have regarded this pressure as certainly produced in the way thus suggested, and that its appearance at the surface has been due to an enormous erosion which has denuded the overlying blanket. This conclusion is not necessary; for, 1°, an enormous pressure would result from the tangential force of contraction, which would be entirely adequate to have produced the cavities. 2°. The necessity of an erosion of 40,000 feet over all the granites in every part of the world cannot be maintained. In North America, for example, it would necessitate the supposition that nearly eight miles' thickness of rock had been removed from one-fourth of the surface since the Laurentian, for the blanket removed would have equalled in dimensions the crystalline areas. The mere statement of the amount of denudation required refutes the theory. 3°. By reference to existing volcanoes, it is plain that a column of lava will often be adequate to exert the needed pressure. Teneriffe rises 12,000 feet above the ocean, and its cone descends 18,000 feet more to the submarine plateau. When the crater is full of melted lava, there must be a pressure of 30,000 feet at the base of the cone: hence the lava from the reservoir supplying Teneriffe might exhibit the vacuities produced by a pressure of 30,000 feet without any weight above the peak.

When molten lava pours down the side of a crater, the included vapors and liquids must disappear because of the removal of the pressure; but, after a substantial crust has formed, the peculiar markings imprinted at the great depth would remain: hence we can understand how it is that the vacuities are to be seen both in granites and lavas that have been subjected to great pressure. At the Boston meeting of this association I endeavored to show that there are mountain masses of granite in New England possessing all the physical characteristics of volcanic cones. The material must have been liquid, hot, ejected from a vent, and flowed over a plateau, building up a cone, and indurating the underlying floor. It was claimed that such phenomena could be explained only by supposing the granite to have been erupted just like lava. This granite contained the usual vacuities indicative of great pressure just as they are also found in the lava of Monte Somma or the trachyte of Ponza.

When one examines the interior structure of modern lava-flows, he is surprised to find beds nearly as well defined as the foliation of schists. Around vents like Vesuvius or Etna the lava accumulates naturally in quaquaversal sheets, no one eruption being very extensive. When steam and hot water are copiously supplied from the caldron, there may be flows of hot mud and tufa. The closing phases of eruptions are usually showers of ashes falling upon the cone or beyond. If the vent is beneath the ocean-level, the lava is minutely subdivided, and the deposit will be like sand or gravel. Between the igneous flows the ordinary aqueous agencies will wear off excrescences, and scatter the fragments down the slope. These various agencies will produce a concentric stratiform arrangement in the whole mass. Where the eruption is massive, a similar set of layers will be formed.

This mass of volcanic material will be very susceptible to metamorphic influences when placed under the proper conditions of heat and pressure. As the result, new minerals will be formed, arranged in foliated beds or schists. Thus briefly stated may be the origin of foliation. So long ago as 1825, Poulett Scrope advocated essentially this doctrine for the arrangement of the crystalline particles in the crystalline schists, having found an analogous structure in certain volcanic accumulations.

Sufficient has now been said in advocacy of our doctrine that the first land consisted of volcanic islands. This was the Laurentian or azoic accumulation. Cartographers have not yet distinguished the several crystalline deposits, so that it will not be practicable at present to point out the supposed volcanic areas of the Hastings, Grenville, Montalban, Huronian, and other eozoic periods. Sedimentation would also act so that in this age many beds must be referred to an aqueous derivation. By the close of the eozoic the continent was outlined; or at least the framework of the future superstructure was put into position. The broader patches about to be mentioned had their origin in the earlier numerous islands cemented by detrital accumulations.

The more important areas developed in the eozoic must have been Greenland, Canada east of Lake Winnipeg, the Atlantic district, the Rocky Mountains, the Sierra Nevadas, and numerous buttes over the Cordilleras. The three great depressions of Hud-

son's Bay, the Mississippi valley, and the Salt-Lake and Nevada basins commenced to sink very early, and the future growth of the continent consisted largely in filling them up with marine sediments. An inspection of a map drawn upon a correct scale will dissipate the fancied resemblance to the letter V, in the Canadian dominion, so often insisted upon. Neither has the development of the land been in bands parallel to the north-west and south-east arms of this supposed angle. A better conception would find three great basins, excluding the unknown regions of Mexico and Alaska, in each of which operations were conducted independently. The best known is that of the interior of the United States, or the Mississippi hydrographic basin. This depression was nearly encircled by a crystalline border of high land. Beginning at Alabama, we follow it to New England, thence by a slight gap to the Adirondack promontory, thence across the Lakes to the Dakota promontory. In Minnesota and Dakota the schists are more or less covered by cretaceous clays and tertiary sands; but they evidently constitute the floor for the surface strata occasionally piercing through the later deposit, as in the Black Hills. Thus we may connect the Dakota and Rocky Mountain crystallines. From Wyoming southerly the granites are again conspicuous into New Mexico. Thus the circuit is not complete: it is like a horseshoe, with the lower Mississippi valley in the gap; yet this may have been filled in the Cambrian age, since Laurentian islands are found in Texas, Arkansas, and Missouri. We might give reasons for believing in the recent origin of the depression between New Mexico and Alabama.

The map will show, around the borders of this Mediterranean Sea, the primordial sea-beach, whether examined in Virginia, New York, Michigan, Colorado, or Texas. Could we dissect the land, we should find an immense platter of Cambrian sediments coextensive with the crystalline highlands surrounding and underlying it. In Cambro-Silurian times the story is repeated. Marine limestones formed other dishes, each limited in size by the upturned edges of the platter underneath. The rest of the history is given in our text-books. Our Mediterranean Sea was not closed till the end of the cretaceous, when the salt-water was expelled, never to return.

In the west a similar ovoidal, crystalline border can be traced, holding paleozoic sediments. Beginning at the Rocky Mountain chain in Wyoming, we follow it southerly to Mexico. Across Arizona are many gneissic outlines, but not sufficiently numerous to close the gap. In California we reach a country entirely gneissic beneath the sands of the desert, which connects with the Sierra Nevadas, and is traceable along the Nevada line nearly to Oregon. There the course is changed, the rocks trend north-easterly, show themselves conspicuously in the Blue Mountains of eastern Oregon, the Salmon River Mountains of Idaho, and western spurs of the Rockies again in Montana, which are continuous to our starting-point in Wyoming. Our crystallines do not pass north of the parallel of 49° into Columbia. We have therefore found a complete crystalline border for the depressions of our western territories, and, within this ovoidal line, all the members of the paleozoic, mesozoic, and cenozoic groups, but not arranged with the simplicity of their distribution in the east.

Less is known of the arctic basin than of the others; but the scattered sketches afforded by voyagers indicate the presence of the more important members of the geological column. Where these basins adjoin, there is a much wider area of ancient land.

In conclusion, I will simply recapitulate the more important phases of the growth of our continent.

We start with the earth in the condition of igneous fluidity.

It cools so as to become incrusted and covered by an ocean.

Numerous volcanoes discharge melted rock, building up ovoidal piles of granite, which change gradually into crystalline schists. When these hills are high enough to overlook the water, they constitute the beginnings of dry land.

At the commencement of paleozoic time the continent is composed of three immense basins, located near Hudson's Bay, the Mississippi hydrographic area, and the great Nevada series of land-locked valleys.

The later history of the development of the continent presents the details of the filling-up of these depressions, the expulsion of the Mediterranean seas, and the description of the varied forms of life that successively peopled the land and water.

The history opens with igneous agency in the ascendant. Aqueous and organic forces became conspicuous later on, and ice has put on the finishing touches to the terrestrial contours. The completed structure we must acknowledge to be 'very good.'

NOTES AND NEWS.

Our leading article of June 29 was based in part upon a mistake, which we desire to correct. Foreign periodicals received by mail in single numbers have not been dutiable within the last five years. Nevertheless, the writer of the article, who subscribes to three foreign scientific journals, and receives them by mail, had been forced to pay duty on each number for the past nine or ten months; and the same has been the case with others of our acquaintance. Our post-office regulations are so frequently changed that one can rarely tell whether he is the victim of a blunder or a whim.

— M. Pasteur, who has just obtained a grant of fifty thousand francs from the French Chambers to send a scientific mission to Egypt to investigate whether the cholera be not due to the development of a microscopic animal in the human body, states, in a letter to *Voltaire*, the reasons which induced him to recommend the board of health to send out the mission in question. He says, "I urged the sending-out of this mission on account of the great progress that science has made since the last cholera epidemic respecting transmissible diseases. Every one of those

diseases that have been the subject of a thorough investigation has led biologists to the conclusion that they were caused by the development, in the body of man or the animals, of a microscopic animal, causing therein disturbances frequently fatal. All the symptoms of the disease, all the causes of death, are directly under the influence of the physiological properties of the microbes. What is needed at present to meet the requirements of science, is to ascertain the primary cause of the scourge. Now, the present state of our knowledge indicates that we should direct all our attention to the possible existence in the blood, or in such or such an organ, of an infinitesimally small being whose nature and properties would in all likelihood account for all the peculiarities of cholera, both as regards its morbid symptoms and the mode of its propagation. The existence of that microbe once ascertained would speedily settle the question as to the measures to be taken to check the spread of the disease, and might possibly suggest new therapeutic means to cure it." The mission consists of four young *savants*, doctors, and biologists, — Drs. Roux, Thuillier, Straus, and Nocard. M. Pasteur hopes, that, by scrupulously attending to the hygienic precautions he has written down for them, the great danger they are incurring may be minimized.

— The September *Century* has several papers to which our readers' attention may be called. One of the illustrated articles relates Lieut. Schwatka's personal adventures in the hunt for the musk-ox. Ernest Ingersoll gives an excellent account of Mr. Agassiz' private laboratory at Newport, and of the methods he has so successfully introduced for carrying delicate sea-animals through their earlier stages. An admirable portrait, engraved by Velten, from a photograph of Notman's, will interest many. It has more spirit than one formerly published in the *Harvard register*. Under the title, 'The tragedies of the nests,' John Burroughs writes of the difficulties birds encounter in rearing their young. The attempts toward the unification in railway time in this country are briefly discussed by W. F. Allen.

A writer on ornamental forms in nature gives several striking illustrations of the effects producible, with due study, by 'the naturalistic school' of decorators. With eyes capable of seeing the stream, moth, vine, and skunk-cabbage 'in nature' as they appear to our writer, we may doubt the possibility of their evolutionary limit in art being ever reached. Like the Spanish-Moorish designer, he 'evidently did not care three straws for what all the botanists and florists on earth might think of his work,' so long as it teach us to regard nature from the standpoint of art, and tend in some measure to straighten the devious paths of the modern conventionalizer.

— The *Tribune* of Minneapolis, for Aug. 16, printed Dr. Dawson's address before the American association in full, as well as long abstracts of several of the sectional addresses. Subsequent issues gave very fair reports of the papers read.

— The first number of Kobelt's *Iconographie der schalentragenden europaischen Meeres conchylien* has appeared. It is in quarto, with colored plates, and this number is devoted to species of Muricidae. The descriptions are in Latin, with German text.

— The Washington, of the Italian navy, under command of Capt. Magnaghi, is engaged in its annual cruise for the study of the western Mediterranean.

— One of the Akkas (African pygmies) taken to Italy in 1873 by Miani has just died of consumption at Verona.

— The newspapers of yesterday announce that Mr. J. A. Ryder has succeeded in rearing the American oyster from the egg. His experiments were made in natural enclosures, and so conducted as to preclude any doubt that the spat obtained has been derived from any source except that of the spawn artificially fertilized and introduced into the enclosure. The greatest obstacle to the cultivation of the oyster is now removed.

RECENT BOOKS AND PAMPHLETS.

Delogne, C. H. Flore cryptogamique de la Belgique. livr. i.: mousses. Bruxelles, 1883. 8°.

Delpino, F. Teoria generale della fillotassi. Genova, 1883. 345 p. 4°.

Depérais, C. Hygiène publique: nouveau traitement des cadavres ayant pour but la destruction des germes contagieux qu'ils peuvent contenir. Naples, *Inst. roy. d'encouragement*, 1883. 19 p., pl. *autogr.* 8°.

Drinker's Explosive compounds and rock drills. Forming a supplementary volume to the first edition of Drinker's Tunnelling. N.Y., 1883. 4°.

Duclaux. Microbiologie. Paris, 1883. 908 p., 111 fig. 8°.

Gerland, E. Der leere raum, die constitution der körper und der aether. Berlin, 1883. 8°.

Grindon, L. H. The Shakspeare flora. Guide to all the principal passages in which mention is made of trees, plants, flowers, and vegetable productions. With comments and botanical particulars. Manchester, 1883. 330 p. 8°.

Henrivaux, J. Le verre et le cristal. Paris, 1883. atlas, 26 pl. 8°.

Heriz, E. Construcción de mapas. Barcelona, *Ramirez*, 1882. 12 p., 8 pl. 4°.

Herrmann, G. Der reibungswinkel. Aachen, 1883. fig. 4°.

Heukels, H. Schoolflora van Nederland. Bewerkt naar O. Wünsche's Schulflora von Deutschland. Groningen, 1883. 62+368 p. 8°.

Israels, A. H., en **Daniëls**, C. E. De verdiensten der hollandsche geleerden ten opzichte van Harvey's leer van den bloedsomloop. Utrecht, 1883. 143 p. 8°.

Jordan, D. S., and **Gilbert**, C. H. Synopsis of the fishes of North America. Washington, 1883. 1,018 p. 8°.

Jordan, W. L. New principles of natural philosophy. London, 1883. illustr. 8°.

Koehler, R. Recherches sur les echinides des côtes de Provence. Marseille, 1883. 167 p., 7 pl. 4°.

Kohlfürst, L. Die elektrischen einrichtungen der eisenbahnen und das signalwesen. Wien, 1883. (elektro-techn. bibl., xii.) 288 p., illustr. 8°.

Lambert, E. Traité pratique de botanique. Propriétés des plantes, leur utilité et leur emploi dans la médecine, l'industrie, etc. Paris, 1883. illustr. 8°.

Larden, W. School course on heat. N.Y., 1883. 321 p., illustr. 8°.

List of British birds. Compiled by a committee of the British ornithologists' union. London, 1883. 258 p. 8°.

Lubbock, J. Fourmis, abeilles et guêpes. Études expérimentales sur l'organisation et les moeurs des insectes hyménoptères. 2 vols. Paris, 1883. illustr. 8°.

Mann, L. Die atomgestalt der chemischen grundstoffe. Berlin, 1883. illustr. 8°.

Martini, A. Manuale di metrologia, ossia misure, pesi e monete in uso attualmente e anticamente presso tutti i popoli. Torino, 1883. 912 p. 8°.